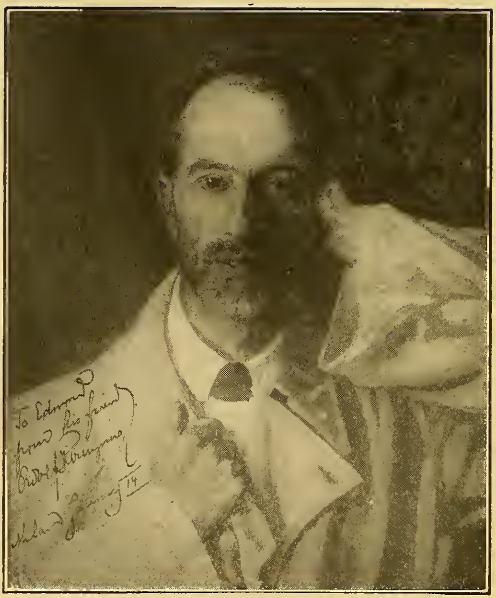
SERIVALOR

THE VALUATION OF RAW SILK

ROSENZWEIG



ADOLF ROSENZWEIG

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The Valuation of Raw Silk

By ADOLF ROSENZWEIG

First English Edition, 1917

Re-written, Revised and Enlarged
to include the most recent
experiences of the Author

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INTRODUCTION

IN presenting to the silk industry of America the only English translation of "Serivalor; the Valuation of Raw Silk," revised and rewritten by Adolf Rosenzweig, the author, the publishers feel that they are rendering a service that will meet with the appreciation of silk men and particularly of the student of silk. Mr. Rosenzweig has an international reputation as an authority on silk. He has devoted the greater part of his life to research work and experimentation in determining the true valuation of raw silk. His "Serivalor" is not a theory, but a practical and logical system of standardizing raw silks, and it was established by him only after years of work along the most practical and scientific lines.

A growing interest in Mr. Rosenzweig's work was aroused by the publication of the essays awarded prizes in the Prize Silk Essay Competition conducted by the Silk Association of America in 1914. In some of these essays Mr. Rosenzweig's work was several times quoted from and referred to. It was then learned that certain firms were so much interested in securing "Serivalor" for use in their own organizations that they were proposing to have an English translation made from the last foreign edition, long out of print and copies no longer obtainable.

Believing, therefore, that Mr. Rosenzweig's work should be rendered available in English for the use of American silk manufacturers, the publishers arranged with him for this revision and in agreeing thereto Mr. Rosenzweig welcomed the opportunity to include in his new writing of "Serivalor" the fresh experience he had obtained in the ten years since the publication of the last edition.

"Serivalor" in its new revised English form was first published serially in *The American Silk Journal,* beginning in that publication July, 1915, and concluding in the May, 1916, issue. The author's final revision is here presented in the belief that it will not only be a valuable acquisition to every silk man's library, but that it will prove of practical value to every student interested in the standardization of raw silk.

Mr. Rosenzweig was writing this revision of his

book at his home in Milan, Italy, during the first year of the European war and was later forced to close his Laboratory Serivalor and take up temporary residence in Switzerland, being a native of Austria.

LABORATORIO SERIVALOR, MILAN.

Editor AMERICAN SILK JOURNAL.

Dear Sir:—In answer to your proposition to publish an English translation of my book "Serivalor," I want to say: The book consists of a mathematical and an empirical part. While the former is, of course, as valid as ever, I feel that the second part can be enlarged by the inclusion of the experience I have had since the publication of my book. I am ready therefore to rewrite the book for your Journal, making use of the studies and experiences I have had during the last ten years. In my manuscript to you I shall therefore bring the work up to date and heighten its worth considerably. Of course, silk inspection cannot be taught by writing only, any more than can weaving or swimming, but the book should become an indispensable guide to all of those who are interested in the matter. Herewith I hand you the prospectus and first chapter of "Serivalor."

Believe me, dear sir,

Yours most faithfully,

ADOLF ROSENZWEIG.

PROSPECTUS

WHEN, after ten years' studies, I was called in the year 1902 to the managing of a broad goods manufactory and to the buying of raw silk, I tried, first of all, to find an expert whose experience would enable me to confirm my theory. This, however, was not easy. There were some who told me of others that knew more than themselves, but when I addressed myself to them, they again pointed out others of superior experience. I proposed the following trial: Between the best and the lowest grades of raw silks there is a difference in price of about $2; dividing this difference into forty degrees of five cents each, it follows that there must exist at least forty different qualities. Having before you ten bales of the same color, could you

give to each of them a distinct number of quality? And when, after a week, I should lay before you other samples of the same bales, would you call quality twenty again what you called it before? Or, how far would you differ? By two, by three, by five degrees? All those to whom I addressed myself told me that this was an impossible task.

Of the truth I was informed by an Italian reeler who is considered to be one of the best experts in the land of silk, who said: "We are judging raw silk according to certain outward characteristics: purity, color and the luster of the thread, and a certain elastic resistance of the skein against the pressure of the hand. By these marks I can recognize the origin of the material, and, as I know by long experience which provinces are producing good and which low qualities of silk, I am able to judge the quality with a certain probability. But there is no absolute certainty. You are asking me whether my 'Extra' quality may be safely used through reeds of a certain fineness. How should I know? I am no weaver, and all I can say is that I am having it reeled out of the best cocoons, always of the same origin, and by well-trained reeling girls. And as my clients are satisfied I must suppose that I am giving them as good silk as they can expect." I asked him whether this way did not lead to occasional disappointments. He said it did. Every new crop or new crossing of breeds, brings about a new uncertainty. It is true that the thread of very bad cocoons will

break during the reeling. But this is not sufficient basis for sharper distinctions. "For the final certainty we must rely on the manufacturer. If he does not complain, we know that the silk is good."

It results, therefore, that the valuation of silk actually is at this point—that the weaver is relying on the reeler's knowledge, while the latter is relying on the weaver's judgment. In fact, if a manufacturer was offered the same material that he pays a fancy price for at the reeler's, at a much lower price by a third person, he would not dare to buy it unless it could be proved by a trade-mark that it really is the same material. Nay, the reeler himself would not be able to recognize his product with certainty if the trade-mark should be wanting. Both of them, therefore, are not judging the material itself, but are dependent on outward signs. It is easily to be imagined after this of what use inspectors can be who for the most part lack the experience of the reeler as well as that of the weaver. The impossibility of recognizing the quality is also an explanation of the fact that even honest reelers are unexpectedly producing bad silk, and continuing to produce it until complaints from their clients make them start from their unconsciousness.

If the manufacturer has no certainty about the quality of silk, neither has he of the quantity. It is of no use to him that the Conditioning Houses are giving him the exact measure by which he buys, the weight, when they are not able to give him at the same time the

exact relation of this measure to the one he is selling by, the length; or, in other words, they are not able to give him the exact size. If a bale declared to be of size 14.00 by the Conditioning House is in reality size 14.55 (and it will be shown later on that a similar error of four per cent. is quite common), the manufacturer will be able to warp only ninety-six instead of 100 warps that the real size 14.00 would have yielded, and when the bale is consumed, he will have lost as much as if he had received only ninety-six for each 100 pounds he has paid for. The annual loss to the American silk manufacturers resulting from this fact may be calculated to be at least a million of dollars, as will be shown later on. Another consequence of the uncertainty of size is that sometimes the pieces of goods sent out in execution of an order are eight per cent. lower in quality than the sample piece, the latter having been woven of real size 14.50; for instance, the former may be 13.50, while the raw material for both was declared size 14.00 by the Conditioning House. What manufacturer has not suffered material and moral losses by such experiences?

Of all these circumstances I had been well aware in 1902, and I was sure of having found a system by which I could determine the real quality and the real size of silk. In 1904 I published a book entitled "Serivalor" and in 1907 established in Milan a homonymous laboratory, exclusively for this task. Invited now to publish an English translation of my book, I want to

enlarge it by the experience of these later years. In the year 1902 I was only a weaver, though one that was making use of scientific methods. In the meantime I have become a reeler too. Of many facts of which I had only been aware of the effects, I have been able to recognize the causes, and the new revised edition of my book will profit by this, my enlarged experience.

In the first chapter I am going to explain thoroughly the important question of size and to show the way by which everybody may repeat the sizing of the same bale a hundred times and more without having it present, and without costs. One will see by what laws the matter is governed, and will recognize that the actual error is, in the average, of about four per cent., while the system "Serivalor" reduces it practically to 0. Passing to quality, it will be shown that the differently named defects of silk may be considered as various forms of the same original defect, but that nevertheless it is necessary to examine them, with regard to their effects on the loom, from seven different standpoints. The methods by which the Laboratory Serivalor is executing these examinations will be explained, and it will be shown how silk is classified with regard to each of these seven items in such a way that the best receive degree 1'0, the worst 10'0, while the intermediate nine degrees are divided into tenths, so that in the whole we have a gradation of ninety tenths of degrees. After this comes the difficult task of uniting

the very often contradicting seven single results into one final number; the "Resultant" expressing the commercial value of the tested silk: An aim which the laboratory has been able to arrive at only after many years of trials and labor. It will be shown, furthermore, how from the "Resultant" there can be derived the forty different qualities of which we spoke in the beginning, and how by this there can be calculated not only whether a bale of silk is "Double Extra," "Extra," etc., but also what price it is really worth according to the last quotations. Five per cent. of the cost price may be gained by this exact qualification only. A still greater advantage will be derived by the manufacturer from availing himself of this principle: Every silk is good enough for the purpose corresponding to its qualities. But even less good silk will give as much satisfaction as the best, if employed in the right way, that is to say, for what it is fit.

The testing being made from seven different points of view, and the silk being in this way, as it were, photographed, the buyer is able to choose the kind that possesses the qualities necessary for his purpose, without being obliged to pay for those that are useless to him.

It will be demonstrated at the same time why the methods of testing silk heretofore used cannot possibly yield any satisfactory result, and that progress is impossible under those methods. It will be shown how deceptive the so called "Elasticity" (in reality, duc-

tility) is, and how little value is to be given to tensile strength. Finally, there will be given some hints which, though not belonging to the main subject, of the book, may still be useful. For instance—

On the increasing of size through the shortening of the thread by throwing.

Regarding the causes of the "lousiness" of silk as manifested after dyeing.

About employing the various qualities of silk according to the fineness of the reeds.

The right calculation of the cost-price of tissues, and how by this it appears that many silks that cost less by the pound are in reality more expensive than others costing more.

Differing from most books about silk that sometimes repeat wrong indications from older handbooks without painstaking and expert analyses of them, this book is founded exclusively on my own experiences together with a thorough knowledge of the respective silk literature.

Conscious of the fact of writing for busy readers, I shall try to be as brief as is consistent with my subject, and considerate also in treating matters for the comprehension of which some knowledge of mathematics is necessary. Where this is lacking, however, even long explanations would be of no avail. Such parts are followed by "practical hints" for the use of the merely practical man.

CHAPTER I

THE SIZE (LE TITRE)

WE TOLD in the introduction how important it is for the manufacturer to know the exact proportion between weight—the measure by which he buys—and length—the measure by which he sells. This proportion is expressed in the size.

It is established by international conventions that size is to be considered: "The weight of 450 meters (about 500 yards) expressed in half decigrams." This formula sounds rather awkward, and it contains, moreover, the germ of an error that influences the logical consequences of sizing, as will be demonstrated later on. Expressed in a clearer way, the formula is: "Size is the weight in grams of 9,000 meters (about 10,000 yards)."

The object of sizing is, therefore, to indicate with

an exactitude within the narrowest limits of variation the number of meters, or yards, contained in the bale tested. But as such a bale contains, considering only the usual sizes of 8/10 to 26/28, from 33 to 100 millions of meters, it is obviously impossible to measure them. On the other hand, the weight of the thread changes not only with every 450 meters, as might be supposed from the official standard measure, but with the shortest length, as the following experiment indicates: Take a thread of about a yard's length, divide it with greatest care into two exact halves, and weigh these on a common chemical balance of 1/10 miligram's sensibility. *The two halves will show different weights.* But there are chemical scales of 10 to 100 times finer sensibility, by the aid of which one can repeat the experiment with pieces 10 to 100 times smaller, and the weight of each piece will always differ.

By this it appears evident that the length of a great quantity of thread cannot be fixed by mere measuring and weighing, but only by a combination of these with mathematical methods known as the Theory of Probabilities. That our ancestors who created the rules of sizing were not acquainted with this theory is not their fault. But that they did not consult a mathematician in this difficult matter, they may justly be reproached for by the manufacturer. For it is he, and not the reeler, the throwster, or the tradesman, who has to suffer the immense loss consequent on this uncertainty.

It can hardly be the object of this book to teach the Theory of Probabilities but I shall try to give all the hints useful for the comprehension of the matter in hand. The studying of these elementary rules, and especially of the fourth, regarding "Constancy," is indispensable to anyone wishing to solve the problem of sizing.

ELEMENTARY RULES FOR CALCULATING PROBABILITIES.

Throwing up a coin ten times and finding that seven times it fell head and thrice tail, I evidently would commit a gross error if therefrom I should conclude that the coin will always show more than twice as many heads as tails. This error would be the consequence of an inadequate number of trials.

First rule: Tests, whose results lay in the realm of Probabilities, must be repeated many times.

Repeating the experiment 1,000 times, I get 498 heads and 502 tails. Does this prove that the coin has a tendency to fall on the tail side? No!

Of this follows the

Second rule: Attempts at probabilities do not yield as clear figures as arithmetic calculations.

Comparing the sums of each ten trials, I find that now the heads and now the tails are prevalent. From this I conclude that the coin will fall as many times

on the tail as on the head side, and this result is quite as exact as that of any arithmetic calculation.

Third rule: The calculation of probabilities gives as reliable result as any arithmetic procedure, if these results are logically worked out.

Fourth rule: The results of probabilities are to be considered as exact, if on essays repeated many times those results differ only within narrow limits from their average, that is to say, if they are *constant*. The interval between the extremes indicates the number of trials to be made.

Till now we have worked with the object having two sides and arrived at the true after about 100 trials. Proceeding however to a die, which has six sides, we find that 100 essays yield no constant results. Of this follows the

Fifth rule: The number of essays necessary for finding that which is true increases with the number of possibilities.

The third rule proves that a sizing sufficiently exact for practical purposes is possible; the fourth that the methods hitherto used are inadequate; the first indicates the reason thereof. The fifth shows that regular and irregular threads, when treated with the same methods, will give results of different exactness.

Let us now return to practical experience, and let us suppose that the owner of a bale of silk accompanies

this bale to the Conditioning House, demanding that the testing should be done in his presence so that he may assure himself of its exactness. The director explains to him the difficulty of the task; the silk thread is considerably lengthened by tension and nevertheless it is necessary to stretch it, in order to measure it. It must be dried out to 0° of humidity and weighed on a very sensitive balance; this balance must be regulated daily, as it might have changed during the night, etc. He then draws *one* skein out of the bale and with the greatest possible care measures off *one meter*. After having dried, weighed and calculated it, he declares the bale to be of size 9.

The owner of the bale is surprised. He has bought it as 13/15, and he possesses experience enough to judge by the mere touch of the skeins that it can hardly be of size 9. But the testing has been done in his presence with care and accuracy, and he does not know yet where the error lies. He takes away his bale, and returns the next day with another one and makes the same request. Again the procedure is repeated with the same conscientious care; result: The bale is declared to be of size 18.

The owner now confesses that he has twice brought the same bale for testing and does not conceal his discontent. The director replies coolly that he had performed the testing according to his prescriptions and with the greatest possible exactness, and could do no more.

The reader has long before recognized where the cause of the error lies; in the insufficiency of the length measured. And how has it become evident that the results are wrong? *By the vast difference between them.* But is he persuaded that there is no difference between the results of the actual official sizing, that is to say, that they are *"constant?"* Of course, the Conditioning Houses are not measuring one meter out of one skein, but 20 to 30 times 450 meters out of 5 to 10 skeins (the respective rules are different in different places) that is, from 9 to 13'5 kilometers. Why just this quantity? Why not 1 km. or 2, or 5, or 20? Why just 5 or 10 skeins, why not 2 or 15? It is generally supposed that, in fixing the rules by which the Conditioning Houses are obliged to work, a quantity was adopted that could guarantee the constancy of the results. But this is not the case.

In the German and French editions of my book, of 1904, I proved the unreliability of the actual methods of sizing. On the occasion of the International Silk Congress of Torino, 1911, the Milan and Como Conditioning Houses published a pamphlet: *"Quelques remarques . . . sur les moyennes et les écarts de titre des grèges"* in which they show by the trials made on more than 30 bales, how inconstant their indications are, not only in regard to size, but also in regard to the *"écart,"* that is, the distance between the lightest and the heaviest sizing skein, which distance generally, though wrongly, is considered as a measure of the

regularity of the thread. Not only the seven Japan bales tested but also the 27 Italian bales, among which there were 13 Extra, showed vast differences, for the same bale, in regard to the size, the extremes, and the reeling. Table 7 of the pamphlet, for instance, gives the results of an Italian Extra bale, with fluctuations in averages of sizes between 13.70, 13.73, 13.83 on one hand, and 14.76, 15.00, and 14.00 on the other. The extremes vary between the minima of 11½, 12, 12½, and 13, and the maxima of 15½, 16, 16½, 17, and 17½. The breaks in the reeling vary between 0 and 5, etc. Other bales show greater variations still. On table 25, for instance, (Italian *1er ordre*) the breaks vary between 6 and 20! On table 28 (Japan 13/15, 1½) the averages of size oscillate between 12.9 and 14.4 (that is, nearly 12 per cent.) the minima between 9 and 11½, the maxima between 14½ and 19, so that a casual combination of 11½—14½ (*écart* 3 den.) on the one hand, and 9—19 (*écart* 10 den.) on the other might have been possible.

The pamphlet arrives at this conclusion:

"1. *La plus grande partie, ou même la presque totalité, des soies qu'on accepte comme regulières, ne le sont effectivement pas.*

"2. *L'essai, comme il est pratiqué aujourd'hui, sur un très petit nombre de flottes, est un véritable jeu de hasard.*

En pensant, que de telles conclusions sont la résultante d'un grand nombre d'essais sérieus et incontestables, nous nous demandons, s'il est juste, et même honnête, de continuer avec ces systèmes."

In English:

"1. The greatest part, nay, it might be said, nearly all the bales of silk that are accepted as regular, are not so in fact.

"2. The testing, as it is done nowadays, on a very small number of skeins, is *a mere play of hazard.*

"Considering that these conclusions are the result of a great number of elaborate and indisputable tests, we must ask ourselves, *whether it is right, or even honest,* to go on with these methods."

How can this intolerable state of things be remedied? In order to arrive at an answer to this question, let us make the following experiment: We divide 5 kilos of Tsatlee (Gold-Kilin) into skeins of 450 meters and receive 4236 skeins. The real size of the 5 kilos is therefore 23.61. Weighing every single skein, we find all sizes

from 13½ to 42.

Weighing two and two together, we find all sizes

from 15½ to 35½.

We see that the error has diminished on both sides, so we are on the right way.

Taking 4 skeins	(1800 meters)	together, we find all	sizes	from	18	to	31½
" 8 "	(3600 ")	"	"	"	19	"	29
" 16 "	(7200 ")	"	"	"	20½	"	27
" 32 "	(14400 ")	"	"	"	21	"	26

We are now arrived at about the number of skeins, by which the Conditioning Houses establish the size, and we see that the real size 23.61 might, according to the casual grouping of skeins, be declared as anything between 21 and 26. Nay, the difference might be greater still, as in the Conditioning Houses the sizing skeins are taken from a small number of original skeins.

The averages of	64 skeins	(28.8 kilometers)	vary	between	22	and	25
" "	128 "	(57.6 ")	"	"	22½	"	24½
" "	256 "	(115.2 ")	"	"	23	"	24¼
" "	512 "	(230.4 ")	"	"	23¼	"	24

The following table unites the results obtained:

Length measured kilometers.	Skeins.	Sizes obtained.	Greatest difference from real size in % of the latter.
0.45	1	from 13½ to 42	78
0.90	2	" 15½ " 35	51
1.8	4	" 18 " 31½	34
3.6	8	" 19 " 29	23
7.2	16	" 20½ " 27½	16
14.4	32	" 21 " 26	10
28.8	64	" 22 " 25	7
57.6	128	" 22½ " 24½	4½
115.2	256	" 23 " 24¼	3
230.4	512	" 23¼ " 24	2

Comparing the first with the last column, we see that the difference from the actual or true is *diminished by about a third by doubling the length measured.*

Of this there follows that in the present case it would be necessary to measure 460 kilometers (1024 skeins) to bring down the difference to 1.3 per cent., and 920 kilometers (2048 skeins) in order to be sure that it does not surpass 1 per cent.

While, then, taking two skeins instead of one, we have got nearer to the actual by 27 per cent., finally, by taking 2048 instead of 1024, we have approached it only by 0.3 per cent.—an effect that is out of proportion to the work it requires. We arrive, therefore, at a certain point, where the increasing of skeins ceases to be useful, and we call this the *rational limit* of sizing.

It remains now to fix this "rational limit," in order to find out the number of skeins, viz., the length to be measured, necessary for exact sizing. The following table serves for this purpose, and is to be used in this way:

Drawing 30 skeins (of 450 meters) from a bale and choosing the two heaviest and the two lightest ones, we divide the sum of the former by that of the latter; those being, for illustration, 30 :25=1.2. Looking for the quotient on the table, we find that 30 skeins are sufficient only when the quotient is 1.19, or less. We therefore increase the number of skeins to 60, comparing now, however, the four heaviest with the four lightest ones; the proportion being f. i. 58 :48,

the quotient is 1.21, and as the table indicates 1.24, we may be sure to have approached the true within + or — 2 per cent.

TABLE FOR SIZING WITH AN EXACTNESS OF + or — 2 per cent.

Number of skeins.	Being kilometers.	Number of skeins to be taken from each of the extreme sizes.	The result answers the purpose, if the quotient is not superior to:
30	13.5	2	1.19
60	27.0	4	1.24
90	40.5	6	1.28
120	54.0	8	1.33
150	67.5	10	1.37
180	81.0	12	1.42
200	90.0	14	1.47

By practical experience it has been ascertained, however, that in nearly all cases 200 skeins (90 km.) are wanted and that it is hardly necessary to make the preliminary trials. On the other hand there are bales for which even 90 km. do not suffice, but these are of lower quality and therefore rarely pay the increased expenses of sizing. In each case we can find out by the calculation that will be shown later on, how far the true size may differ from the established.

Meanwhile we are going to make another experiment. It would be desirable, no doubt, to repeat the sizing of the same bale, let us say, of Japan 1½, 13/15, a hundred times and more, and learn from the results obtained. It is true that even a hundred sizings do

not give the absolute size of the whole bale, but the average of so many trials would be near enough to the actual for our purpose. But the difficulties of repeating the sizing of the same bale a hundred times are evident. The thing would be much easier if the whole bale consisted of skeins of 450 meters; then we should not have to measure but only to weigh. And the task would become much easier still if to each skein were attached a label indicating its weight; then we need neither measure nor weigh but only read. And pursuing the idea, we find that in this case *we should not want the skeins themselves but only the labels.*

Let us imagine, then, a bag filled, instead of with silk, with labels bearing the sizes contained in a picul of Japan 1½, 13/15, varying, as we know, on one single sizing bulletin from about 11 to 18. (A second bulletin of the same bale may show sizes 10-17 or 12-20.) The bag would contain about 86,000 of such labels, among which, however, the single sizes would not be represented in equal numbers, but, as we can see from any sizing bulletin, from the finest to the middle size in increasing and from the middle to the heaviest in decreasing number. Out of this bag we should then draw 30 labels and calculate their average, by which we should have performed one sizing. But as we know that the bag does not contain the single sizes in equal numbers, we must not leave outside the drawn labels lest we should alter the pro-

portion of the single sizes to each other. This would be allowed only if we contended to exhaust the bag completely, that is to say to perform about 2,800 sizings of 30 labels each. Wishing, however, to do only 100-200 sizings in order to see how one and the same bale is represented by each of them, we must be careful to put back each drawn label so that we might not alter the character of its contents.

Let us now go a step further. Being obliged anyhow to put back any drawn label, the bag need not even contain 2800 x 30 labels, but *we shall arrive at the same result if it contains only one single series of 30 labels,* as this remains unchanged if we put back every drawn label and mix them thoroughly.

In comparison with the sizing of real silk skeins this experiment has not only the advantage of simplicity, cheapness, and quickness, but is also true, as we are in knowledge of the true size of the imaginary bale, which can never be the case with a real bale.

In order then to have the reflex of reality, we need only to write the 30 figures of some sizing bulletin on handy labels, put these into a little bag and begin with the drawing. The bulletin No. 5136, of April 15, 1915, of the Stagionatura Anonima, Milan, reproduced here, might serve as an example:

Report on test made on samples of 10 skeins in raw R C 24 on weight of Kilo 0.705		
	SIZE.	
11½	13½	15½
12	14	16
12½	14	16
12½	14	16½
12½	14	16½
13	14½	17
13	14½	17
13	14½	17½
13½	15	17½
13½	15½	17½

We see that this bulletin contains:

$$\text{Sizes } \frac{11\frac{1}{2}}{1},\ \frac{12}{1},\ \frac{12\frac{1}{2}}{3},\ \frac{13}{3},\ \frac{13\frac{1}{2}}{3},\ \frac{14}{4},\ \frac{14\frac{1}{2}}{3},$$

$$\frac{15}{1},\ \frac{15\frac{1}{2}}{2},\ \frac{16}{2},\ \frac{16\frac{1}{2}}{2},\ \frac{17}{2},\ \frac{17\frac{1}{2}}{3}.$$

Sum: 4375, average 4375 :30 = 14.59.

(Any other sizing bulletin that shows a similar average (about 14.55 to 14.65) with a similar écart (6 den.) would serve as well.)

The experiment, viz., the drawing of 30 numbers should be repeated at least 100 times, writing down each drawn number, but calculating the average not of each 30 but of each 10 numbers, and drawing together 3 averages of 10 to one of 30. This procedure

is more practical, as later on we shall want the averages of 20 x 10 numbers, and so those of 10 will come handy.

According to the Theory of Probabilities among 100 sizings (of 30 numbers each) there will appear: averages up to about—

$$\frac{14.0,}{3}\quad\frac{14.1,}{4}\quad\frac{14.2,}{10}\quad\frac{14.3,}{10}\quad\frac{14.4,}{10}\quad\frac{14.5,}{13}$$

$$\frac{14.6,}{10}\quad\frac{14.7,}{3}\quad\frac{14.8,}{7}\quad\frac{14.9,}{7}\quad\frac{15.0,}{13}\quad\frac{15.1.}{10}$$

Of this follows:

1. Of 100 sizings of a bale of real size 14.6 to 14.7, 50 will be such that the bale may be delivered as 13/15, according to "usage."

2. If therefore I have sizings made on 100 bales 14/15, or 14/16, I may be sure that 50 of them may be delivered as 13/15. Repeating the sizing of the remaining 50 bales, half of these will again appear as 13/15, and I have only 25 bales left, with which I go on in the same way until all the 100 bales have passed as 13/15. How many sizings were necessary for this purpose? $100 + 50 + 25 + 12$ or $13 + 6 + 3 + 1$ or $2 = 200$, that is to say two sizings per bale instead of one.

3. A more practical and more direct way is to

have each bale sized twice at once. Thus we receive two bulletins for each bale, of which nearly always one will be of 13/15. *This twofold sizing is the general useage in nearly all silk-trading places.*

It is therefore a mathematical fact that by the twofold sizing of each bale the real size appears altered by about 4 per cent. How large the loss accruing from this fact is, especially for buyers of Japan 13/15, was shown to me by the sizing of 112 piculs that the Laboratory Serivalor had to effect some time ago. These 112 piculs of Japan 1½ had all passed as 13/15. According to the sizing of the Laboratory, on a basis of 9.0 kilometers (200 skeins), 58 of them appeared as 14/15, *and the average of the whole* 112 *was* 14.58, therefore more than 4 per cent. too heavy. I am sure that the controlling of each great lot of Japan 1½, 13/15, would give the same and worse results. The reason lies in this: In each country the great bulk of cocoons consists of a thread (*bava*) of a certain size which then determines the size of the silk-thread reeled therefrom. In Japan the size of the bava of the main part of the crop evidently is near to 2.45, and therefore to the reeler

4 cocoons will yield	4 x 2.45 = 9.80,	or 9/11
5 " " "	5 x 2.45 = 12.25	" 11/13 — 12/13
6 " " "	6 x 2.45 = 14.70	" 14/15 — 14/16,

but it is difficult for him to arrive at size 14, for which he would be obliged to employ:

or 5 cocoons of 2.80 = 14.0
or 6 cocoons of 2.35 = 14.1,

which kinds evidently are comparatively rare in Japan.

In Italy a bava of 2.8 den. is quite common, and therefore in Italian silk the size 13/15 will generally be right. On the other hand, a great part of Italian 11/13 are in reality 12/13 to 12/14, as a bava of 2.4, of which 5 cocoons would yield size 12, is rather rare, and the reeler must be content to find 2.5 to 2.6 yielding sizes 12.5 to 13. (It would be easy for the reeler to find 3.0, of which 4 would yield size 12; but the reeling of 4 cocoons is very difficult, as we shall see later on, and the thread contains very many fine ends.)

It might be observed here that though every conscientious reeler makes frequent size tests such tests are of no great avail, as the variations in drying the wet thread bring results more or less incomplete, according to the humidity of the surrounding air, and the reeler, of course, does not use a conditioning apparatus. This leaves the reeler uncertain of about 4 per cent. of the size.

How great is the loss to the American manufacturer resulting from these circumstances? The American consumption of silk amounts on an average to 100 millions of dollars a year. We have seen that the twofold sizing of each bale changes the true size as much as 4 per cent. Supposing that this applied to only one out of four cases, which is certainly below

the mark, yet even then we estimate a loss of a million of dollars yearly. (*This loss running through imperceptible holes accounts for many an unaccountable minus in the manufacturer's annual balance.*)

How can this loss be avoided? Simply by exact sizing. Taking as a basis, instead of the usual 13.5 kilometers (30 skeins), 90 kilometers (200 skeins) as the Laboratory Serivalor is doing, the model bale previously utilized for our experiments, will yield in each 100 sizings:

Sizings	7	20	36	17	20
Sizes	14.35-14.40	up to 14.5	14.6	14.7	14.8

(These results may be controlled by any reader. We had previously said that the averages of each 10 numbers should be noted. Writing down these averages on new labels, drawing them in the manner previously explained and taking the averages of each 20, these represent the averages of 20 × 10 = 200 sizing skeins = 90 kilometers, viz., the quantity measured by the L. S.)

We see now how the possible error has dwindled. Only 7 of 100 bales 14/15, and none of 14/16 could be delivered as 13/15, and this small possible gain will induce nobody to have each bale tested twice. But also the deviation of 1½ per cent. (14.35-14.40 instead of 14.59) will occur as often above as below the mark, and therefore be practically reduced to nothing.

How great the error is in each single case may be calculated mathematically out of each series of 10 sizings of 9 km. (= 20 skeins) as done for each single bale by the Serivalor system. The formula, which unfortunately cannot be explained at length here, is the following: *a* being the average of *n* sizings, *d* the difference of each sizing from the average; then we have:

$$R = \frac{100\sqrt{\dfrac{d_1^2 + d_2^2 + d_3^2 \ldots\ldots\ldots + d_n^2}{n-1}}}{a}$$

The resultant: *R* expresses the mean division in per cent. of the size.

Here are some actual examples:

Italian 1st order, 9/11. 10.0, 10.2, 10.2, 10.2, 10.2, 10.3, 10.5, 10.6, 11.2. Average 10.4. Mean deviation 0.33 per cent.

Italian 2d order, 11/13. 11.0, 11.1, 11.2, 11.4, 11.6, 11.8, 12.2, 12.3, 12.6, 12.8. Average 11.8. Mean deviation 1.75 per cent.

Italian Classical 14/16. 14.2, 14.4, 14.6, 14.7, 14.8, 15.0, 15.2, 15.3, 15.4. Average 14.8. Mean deviation 0.30 per cent.

Italian Extra 27/29. 26.5, 27.3, 27.5, 27.5, 27.5, 27.8, 28.0, 28.2, 28.7. Average 27.7. Mean deviation 0.78 per cent.

Japan Double Extra 12/13. 12.0, 12.0, 12.1, 12.2, 12.4, 12.6, 12.8, 12.9, 13.0, 13.0. Average 12.5. Mean deviation 1.3 per cent.

Japan 1½, so called 13/15. 13/7, 13/8, 14.0, 14.1, 14.2, 14.3, 14.5, 14.8, 15.6, 16.0. Average 14.5. Mean deviation 1.7 per cent.

Japan 1½-2, so called 13/16. 13.7, 14.0, 14.3, 14.7, 14.7, 14.8, 15.5, 15.6, 17.0. Average 14.9. Mean deviation 2.0 per cent.

Some of these examples show that for very irregular threads even sizing on a basis of 90 km. is hardly sufficient. In such cases, in which the mean deviation exceeds 1 per cent., the sizing should be made on 180 km.

In any case the result can be exact only if the sizing skeins are taken from a sufficiently great number of original skeins. Of this number which, of course, has also its *rational limit,* we want to say:

A bale of silk is produced either by many reeling girls in a short time, or by a few of them in a longer time. In the first case the number of skeins drawn ought to be equal to the number of reeling girls, in the second to their number multiplied by the number of weeks they had worked at the bale—(supposing that in the worst case their way of working had changed in the course of a week).

For according to the laws of probabilities the result of any new test will be equal to those of the former ones, if the skeins drawn represent the entire character of the bale. And as the latter is the product of a certain number of individuals, it would be de-

sirable that a skein should be drawn for every individual. It is necessary therefore to draw the skeins from all parts and *layers* of the bale. Of course we do not know which is which; but as they are indiscriminately mixed, by drawing them from all parts, we may be sure to get a true characteristic of the whole.

Let us now consider opposite suppositions:

a. The bale (100 kilos) was reeled by one single girl.

With a production of 500 grams daily, the work should require 30 weeks. Therefore 30 skeins would be drawn.

b. The bale was finished in a week. Then 30 girls have worked at it, and we have again to draw 30 skeins.

For these reasons I made the tests first on 30 skeins, and having arrived at satisfactory results, tried to diminish their number to 20. This number has proved sufficient as far as Italian silks are concerned, but not quite as reliable for Asiatic silks.

The Laboratory Serivalor therefore proceeds as follows:

Of each bale (100 kilos) Italian, 20 skeins are drawn.
" " Picul (60 ") Japan, 15 " " "

(We have said that of Italian silks the skeins must be taken from all parts and *layers* of the bale.

With Japans each skein must be drawn from a different parcel.

In testing Japan silks, two piculs are always coupled together and the testing is done on $2 \times 15 = 30$ skeins. These piculs must be considered as a unit and employed together on the loom.

Of each of the 20 (respectively 30) skeins 4,500 meters (= 10 sizing skeins) are measured off and the size established according to their weight at 70 per cent. humidity of the air. The silk measured off this way then serves for all the other tests to be mentioned later. (Anyone who may doubt of the measuring being really done may demand that an additional 90 kilometers should be measured off and sent to him.)

CHAPTER II

SOME REMARKS ABOUT QUALITY IN GENERAL

IF A dozen experts were called, let us say, by a judge in a lawsuit, to answer the question, "What does the quality of silk consist in?" they would in all probability give twelve complicated answers rich with technical expressions, each absolutely different from the other. Dark hints about "a certain touch," "a certain luster," etc., would only serve to hide the want of a clear definition, and after having heard the twelve, the judge would be as wise as before.

In reality, the answer is very simple: *The best silk is the one that allows the highest speed on the loom.* On the loom, and not in winding, warping, or throwing, for these procedures are not so important as weaving. If a bale of silk proved bad in winding, warping or

throwing, but allowed speedy weaving, it is of excellent quality; just as it is of bad quality if the weaving is slow, though the winding, warping or throwing had been excellent.

This, of course, appears evident, as a break in winding and throwing stops one thread only, in warping 300, in weaving up to 15,000. In fact, the wages and general expenses are in the proportion of 1 for winding to 2½ for warping, and 10 for weaving. The speed in weaving is worth ten times that in winding, and four times that in warping.

But there are many people who consider the winding as a touchstone for quality in general. To those I want to say that this supposition is quite wrong, as will be demonstrated in its place.

It is an erroneous supposition as well, that the better silk will yield a better, a more durable tissue. The durability of the tissue has nothing to do with the quality of the silk it is made of, and "Gold-Kilin," for instance, will yield a stuff that will wear at least as well as one made of Italian Extra that costs about 60 per cent. more, the only difference lying *in the evenness* of the surface.

But now there arises a new question. On what depends the possibility of speedy weaving?

The answer is: *On the "uniformity" of the silk thread.*

The silk thread ought to be "uniform." The ideal claim for it is that it should have the same diameter

throughout its whole length. Its "quality" is proportional to its "uniformity," that is to say, the more uniform it is, the better is its quality, and vice versa; and all its differently named defects are only various forms of one and the same defect: *want of uniformity.*

But according to the shape in which this defect appears, its influence is different on the speed of production, and therefore it will be necessary to treat of these various shapes in separate chapters.

In this instance I want only to give some hints about the way in which the reeler must try to arrive at the greatest possible uniformity.

The silk thread cannot be reeled out of one cocoon, but of four at least, better from five of them united. Here follows the application of the first rule: *The number of cocoons must be the same during the whole time of the reeling.*

Within a lot of the same race the diameter of the cocoon's thread is about proportional to the size of the cocoons. Hence the second rule: *The cocoons reeled together ought to be of the same race and of about the same size.*

The diameter of the cocoon thread is very variable, but as a rule it is much thinner at the beginning and at the end than in the middle of each cocoon. This furnishes the

3rd Rule: *The cocoon threads must complete each other in such a manner that about half of their num-*

ber are at the beginning or at the end, while the other half are at the middle.

If several threads are to be united to one of uniform diameter, it is indispensable that each of them should first form a straight line. On the cocoon, however, the thread is laid in circles and loops like the figure 8. Hence the

4th Rule: *The cocoon thread must be stretched to a straight line.*

Owing to its being enveloped in a sort of gum (sericin) the cocoon thread presents a strong resistance to stretching. Hence the

5th Rule: *The sericin must be sufficiently soaked.* But when thoroughly soaked the thread has a tendency to fall off the cocoons by loops and clusters, which are running into the reeled thread, without previously being stretched. Hence the

6th Rule: *The sericin must not be soaked too much.*

The reeler, however, can act according to rules 5 and 6 only when every lot of cocoons is assorted in such a manner that only cocoons of the same texture are employed together. For the less tightly the worm has crossed the thread, the deeper the hot water penetrates, and the more soaked will be the sericin. The chief differences of texture are called "Reali," "Rea-

lini," "Scarti," "Bombaggiati," but there are still numerous subdivisions.

The 7th rule is, therefore: *The cocoons must be assorted according to their texture.*

The assorting, however, is not sufficient for its purpose if the speed of the reeling is not regulated according to the quality of cocoons.

The right speed of reeling, then, forms the 8th rule.

The well stretched thread laying itself alongside its neighbors, does not unite with them into a round cord, but forms only a flat ribbon. Hence the

9th Rule: *The stretched cocoon thread must be united by a concentric pressure.*

The now completed thread is wet and much inclined to sticking, adhering with its neighbors, especially where they rest on the six logs of the reel.

The 10th rule therefore is: *The sticking together of the threads must be avoided.*

To sum up what has been said before: *The more thoroughly a reeler acts according to the ten rules established above, the better will be the quality of silk produced.*

We are now going to consider the consequences of each single form of the original defect in silk, and to show that they are not to be recognized by the methods used hitherto; but that there are other methods that will yield reliable results.

CHAPTER III

REGULARITY

UNDER this title we are going to treat that want of uniformity which becomes apparent in the variations of size. As we said in the preceding chapter, these variations are the consequence partly of the irregularity of the cocoon thread, and partly of its being reeled without observation of the first and second of the rules established there.

This irregularity is considerable, and more so with the thread of larger cocoons than with that of smaller ones. The difference within one cocoon may be expressed by the proportion 1:3; that is to say, cocoons of the average size of 2:8 den., for instance, contain all sizes from 1.4 to 4.2 den., the finest occurring at the end of the thread (whose length is about 500 meters) the thickest in the middle, the middle sizes at the beginning.

From the proportion 1:3 and from the mathematical law of "Combination" it follows that the unit-

ing of five or six cocoons must yield a thread whose variations of size cannot be of smaller proportion than 10:15, and that consequently even an excellently reeled 13/15 must contain all sizes from about 11 to 17. In fact, even in a well-reeled skein of 13/15, of about 50 yards length, we shall find:

of size......	11	12	13	14	15	16	17
about......	3%	10%	20%	40%	15%	10%	2%

This table expresses the smallest theoretical proportion, which, however, is always exceeded in skeins longer than 50 yards. By this we can see how wrong the trade usage is: that a bale of 13/15 allows no greater écart than 5 den.; when in reality it always exceeds 6 den. This error results from the habit of finding the écart by skeins of 450 meters, which écart is not only wrong but highly inconstant, as proven by our experiment of 100 sizings of 30 labels, and which is also to be seen on every page of the publication of the Milan and Como Conditioning Houses, which we referred to in the first chapter.

In order to find out the length that should serve as a basis for calculating the variations of size, it is necessary to look at the work of reeling.

The reeling-girl is spinning 4 to 6 threads at a time out of 30 cocoons, with a speed varying, according to the quality of cocoons, from 100 to 130 meters in a minute. This speed is so regulated that the girl

has time to "cast" a new thread at every break, which breaks average 15 a minute. In the time between one "cast" and the other each of the six threads runs therefore about 120 meters, and this length represents

——=8

15

the average of the defect caused by the reeling-girl. If, then, we want to test the thread with regard to this defect, we must, according to theory, divide it into lengths of 5-10, but not 450 meters; this latter length would serve only if the reeling-girls were working 50-100 times slower than they do, that is to say, if they were nearly sleeping.

In fact, the testing must be done by lengths not over 50 and sometimes down to 5 meters. Even under this condition it must never be based on the extremes, as these are never "constant" even when established by a very great number of essays. There are even mathematicians who do not consider the extremes at all. After many practical experiments the Laboratory Serivalor, however, has come to the conclusion that, for our purpose, the extremes are to be taken into consideration as well as the other elements of testing for regularity.

Continuing now our research for the smallest possible variations of size within the thread, we find that they are limited toward the thick side. A reeling-girl that has an order to reel five cocoons has no honest reason for taking six of them, and the thread therefore

ought to show no greater thickening of the desired size than is justified by the preceding formula of 10:15; nay, the formula must in this case be reduced to 13:15 as the thickening of the cocoon thread is less important than its thinning towards the end. The formula 13:15 is to be understood in this way, that for the average size of 13 the heaviest size ought not to be over 15, in other words: *the thickest parts of a thread ought not to surpass its average size by more than* 15 *per cent.*

This theoretical claim is justified by the fact that threads of this regularity occur, but they are uncommonly rare, which proves that either the cocoons are, in general, not assorted with sufficient care, or that the controlling of the reeling-girls is not as strict as it should be. For the girl will take six cocoons instead of five whenever for a certain time she has allowed the reeling to go on with three instead of five cocoons. As the controlling of size is done by skeins (*provino*) of 450 meters, she wants to bring about the right size of the latter by adding to the size as much as she has lost by her carelessness. This ought to be strictly avoided, but it has become so common nevertheless that it has brought about the adage:

Tra il grosso ed il fino
Sorte il provino.

("What with a thick thread, what with a thin one, springs forth the *provino*.")

Toward the thin side the possible deviation is

greater, for to the natural irregularity of 10:13, or 77:100, are added the unavoidable breaks of the cocoon-thread, which, as we have seen, take away at least 1/5, sometimes 2/5 of the size, as long as they are not mended. (In reeling four cocoons this frequently occurring accident diminishes the size by 2/4=50 per cent., and therefore at least five cocoons should be employed, as mentioned in Chapter I.)

In fact, the variations toward the thin side are in the best case not inferior to 30 per cent., which corresponds to the coincidence that the reeling-girl missed a "cast," while the thread was running 90 per cent. of its regular size.

We have seen, then, that the testing for regularity can be done only by lengths of 5 to 50 meters at the highest. Now it remains to fix the number of these pieces necessary for reliable testing. This number can be found only by experiments. By many of these it has been ascertained that, as a rule, at least 200 and for very irregular threads at least 400 are necessary to obtain constant results.

The calculations to be made with this material cannot be explained here, being intelligible only to mathematicians who know them as the "Theory of the least squares." To any of those experts who may read this I want to say that the greater or smaller difference between the results of the arithmetical and the geometrical series of differences gives a reliable indication,

whether we are in presence of a bale of "Natives" or of "Filature." With the latter the difference is always below 5 per cent., with the former above 5 per cent.

Though a mathematical explanation is impossible in this place, the procedure can be exhibited to the reader by graphic reproductions. Those given here are made for size 13/15 on skeins of 20 yards, and are applicable for this size only. For the conditions of the regularity of the silk thread are such that the thinner sizes can be made neither as regular nor as irregular as the thicker ones. This will appear evident to anybody who reflects upon the irregularities that *must* occur, and those that *may* occur, with four cocoons on the one hand, and eight cocoons on the other. Consequently the distances between the degrees 1-10 of the gradation Serivalor are different for different sizes.

The working girl charged with weighing the little skeins of 20 yards is registering them on a sheet of paper on whose left hand margin the sizes 4 to 33 are printed in a vertical row, while to each of these figures corresponds a horizontal row of numbers from 1 to 60 (see plates). The first skein being for instance of size 14, it is registered by a dash through number one of series 14, the second of size 17, by one through number one of series 17, and so on, until all the 200 skeins are registered.

It is evident, that the more regular a bale of 13/15

is, the more frequently will occur the size 14, and after it 13 and 15, while, the more irregular it is, the less frequently these three sizes will turn up, while those distant from the average will increase proportionately.

Having finished the registering, the dashes will form a triangle, whose basis will be the shorter and whose height the greater, the more regular the bale is, while on the contrary the height will be the lower and the basis the larger, the more irregularly it was reeled. (For all these, and all the following conclusions it is an essential condition that the silk serving for the test should be taken from 20-30 skeins, drawn from all parts of the bale. Carelessly drawn skeins, or a smaller number of them cannot give reliable results.) By comparing, then, the basis to the height, we arrive at a gradation of regularity, and even the aspect of the diagram suffices for giving a general idea of it. This comparison, however, is not sufficient for a gradation by tenths of degrees, as the Laboratory Serivalor is calculating it.

Let us now look at the following diagram, representing "Degree Serivalor" (S^0) 1 of regularity, for size 13/15.

DIAGRAM 1.

We see that the 200 skeins are divided as follows:

Size......	10	11	12	13	14	15	16	17
Skeins....	2	8	30	38	46	46	29	1

DIAGRAM 2.

The diagram, representing S⁰ 2, contains:

Size	9	10	11	12	13	14	15	16	17	18
Skeins	2	4	10	29	34	43	38	29	10	1

We see that the number of the "ideal" size 14 has diminished from 46 to 43, while the basis of the triangle is enlarged by the sizes 9 and 18. But we see also that size 18 appears only once in 200 skeins, which proves that a smaller number of them could have been sufficient only in exceptional cases.

The following diagram represents S⁰ 3:

DIAGRAM 3.

It contains:

Size	8	9	10	11	12	13	14	15	16	17	18	19
Skeins	2	4	4	8	28	34	40	36	25	11	7	1

It is to be observed how the gliding down of the center sizes is increasing, while the extremes are appearing only in small numbers, a proof that no judgment can be based on these alone. Compared with the preceding diagrams, f. i., they are but slightly different; in size 18 the difference is already more sensible (7 to 1) and so on for the other sizes, which demonstrates that the proportion of all sizes must be taken into consideration.

DIAGRAM 4.

On this diagram, representing S⁰ 4, there appears:

Size....	8	9	10	11	12	13	14	15	16	17	18	19	20
Skeins..	2	4	8	16	22	30	38	31	21	15	9	3	1

The greater irregularity is expressed by no new extreme on the thin side, but only by the continual "gliding down" of the central sizes: while on the thick side there appears a new extreme. This is a characteristic of those silks that are nearly too irregular for single weaving. Size 8 consisting only of 3 cocoon threads, even a careless reeling-girl will not go on this way for a long time, and consequently the increasing irregularity expresses itself chiefly towards the heavy side.

The following diagram, representing S° 5,

DIAGRAM 5

contains:

Size....	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21
Skeins..	2	2	6	8	12	22	30	36	28	21	15	9	5	3	1

Incessantly the height is flattening towards both sides and the center size 14 has diminished from 46 (S° 1) to 36.

DIAGRAM 6.

This diagram, representing S° 6, contains:

Size....	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
Skeins..	2	4	6	10	14	20	27	34	24	20	16	10	6	4	2	1

The sizes 10 and 17, forming the extremes of S° 1, and represented there only by (2+1) 3 skeins, are appearing here already in the number of 26; while the

extremes 7 and 22 amount as usual to a small number only.

Diagram 7.

In S° 7 there appear:

Size	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23
Skeins	2	2	4	6	8	14	22	26	32	23	19	14	10	7	5	3	2	1

The reeling-girl has become very careless. She ought to have had a thread of six cocoons, instead of which it was running sometimes with three cocoons (size 6) only, and she made up for this negligence by increasing their number up to 9 (size 23).

Diagram 8.

On this diagram, representing S° 8, we find:

Size	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24
Skeins	2	4	4	6	10	12	22	25	30	22	18	15	9	6	5	4	3	2	1

Toward the thin side the defect can scarcely increase any more, as the reeling can hardly go on with less than three cocoons; while toward the heavy side it is, as it were, without limits. The center size 14 has diminished to 30 skeins.

The diagram of S° 9

Diagram 9

contains:

Size	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25
Skeins	2	2	4	4	4	8	14	22	26	28	20	16	14	10	8	6	4	3	2	2	1

We see that the triangle is transformed into a pyramid with concave sides, a consequence of the fact that also in very irregular threads the extremes and their neighbors occur but in small numbers. The reeling varies now between two and ten cocoons. Nevertheless this is still "Filature." Natives show other characteristics, as we mentioned before.

The following diagram represents the lowest degree, S^0 10.

Diagram 10.

It contains:

Size....	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
Skeins..	2	2	4	4	6	10	16	20	23	26	19	16	13	10	8	6	4	3	3	2	2	1

The defect could hardly increase toward the thin side, as the reeling cannot go on with less than two cocoons; toward the heavy side new extremes have appeared. The center size 14 has diminished to 26 skeins.

Finally we add the diagram of a Native 13/15.

Diagram Native.

Here we have:

Size....	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22
Skeins..	1	1	2	2	7	13	15	16	17	17	18	17	15	14	13	10	6	3	2

Size....	23	24	25	26	27	28	29	30	31	32	33	34	35	36	37	38	39	40
Skeins..	2	1	0	1	0	1	0	0	1	0	0	1	0	1	0	1	0	1

We see that the reeling varies between two cocoons (that moreover were near to their end: size 4!) and 16 of them. The center size has dwindled to 18 skeins. Notice that the series of sizes show several gaps, as the sizes 25, 27, 29, 30, 32, 33, 35, 37 and 39 are not represented at all; nevertheless they are doubtless contained in the bale, but we do not see them, as for the testing of a bale of such irregularity 200 skeins are insufficient and at least 400 of them are necessary.

Of course, the real essay will never be identical with these theoretic diagrams, but its character will be easily recognized by the experienced tester. In general, if the Serivalor methods cannot be easily grasped in their foundations, their application requires neither special studies nor extraordinary abilities, but only calmness and attention.

From what we have said in this chapter, it becomes evident that the reeler must needs remain in the dark about the question how far his silk will prove up to the just claims of the consumer. The reeling-girl ought to be controlled by the forewoman to insure her working with the right number of cocoons, but the forewoman has about 15 preparing and 25 reeling-girls under her control, and each of the latter has, as we said before, 15 opportunities to the minute for becoming careless. There are 18,000 of these opportunities for each kilo of silk, and ere a complete bale is reeled, about two millions of new "casts" should have been made without

loss of time. No control can guarantee this, and a constant and severe training of the girls can give only the hope but not the certainty of success. In fact, every girl works differently every day according to her health, her humor, etc. On the day after one or two holidays the work is worse than on other days. In times of political and social troubles the quality of the thread diminishes rapidly, and the manager of the establishment, to say nothing of the owner, does not become aware of it. Neither has he the possibility of controlling his product or of having it controlled by a public institution. Therefore, silks that get S° 1 for regularity are very rare.

The influence of regularity on the weaving is of two kinds: (a) The production is diminished by the frequent occurring of fine threads. This defect will be the object of the next chapter. (b) The worth of the tissue is diminished by the unevenness of the surface. This drawback is more sensible with the tram than with the warp, as in the latter the thinner or thicker threads are covered by their neighbors, while in the tram they join themselves together and form stripes, sometimes even folds, that may be the cause of severe loss to the manufacturer.

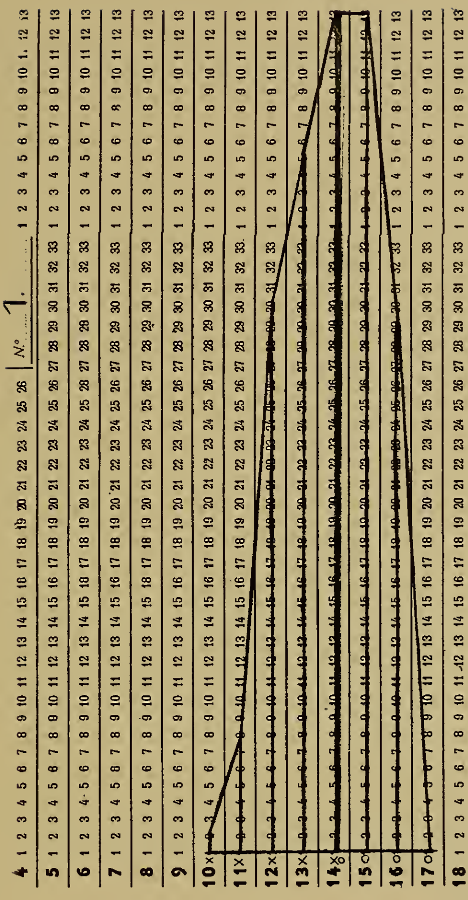
The claims of the consumers to the evenness of the tissue are different according to its character, to fashion and also to the country. Nevertheless, the following table may serve as a general guide:

Not to be employed
a S° of Regularity worse than:

2.0	for	single warps, Malines, 14/15,		
4.5	"	"	" broad stuffs,	
5.0	"	double warps, broad stuffs,		
5.5	"	Organzine	2	threads,
6.0	"	"	3	"
4.5	"	Tram	2	"
5.0	"	"	3	"

Each manufacturer may govern himself according to the claims of his customers, by diminishing or heightening the degrees established here. The chief thing for him is that he may dispose of a system of testing that furnishes *constant* results and that he may be sure to receive always the same regularity under the same designation in Degrees Serivalor, whether he buys the bale to-day or a year hence, whether from reeler A or B, whether it is Italian Extra or Canton Native: he has to judge only by the gradation, and not by origin, time or name.

DIAGRAM 1.



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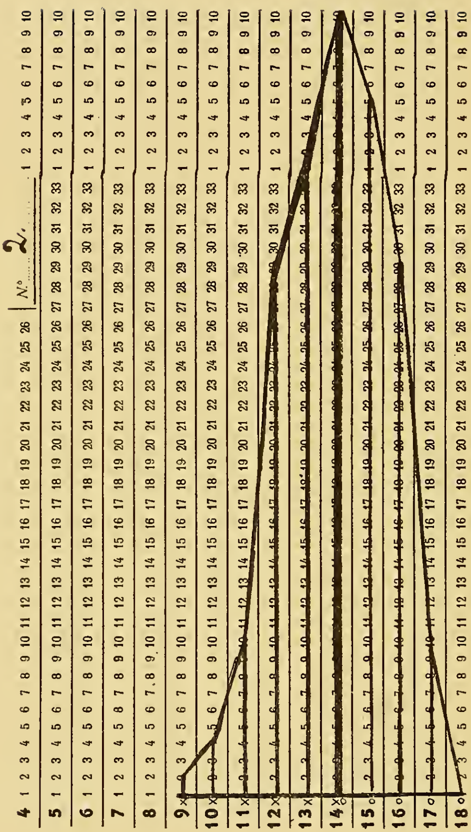

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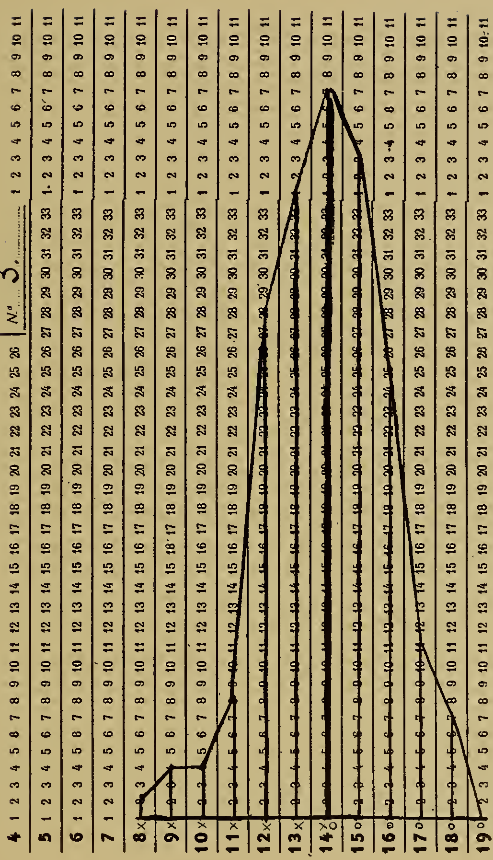

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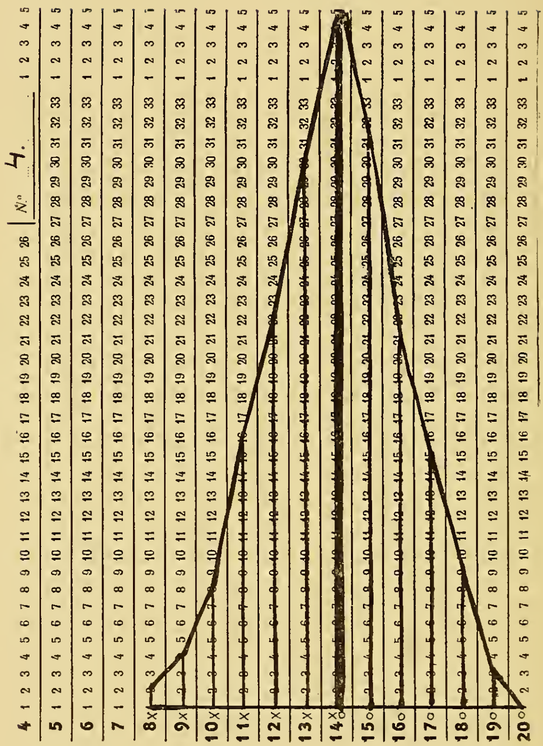

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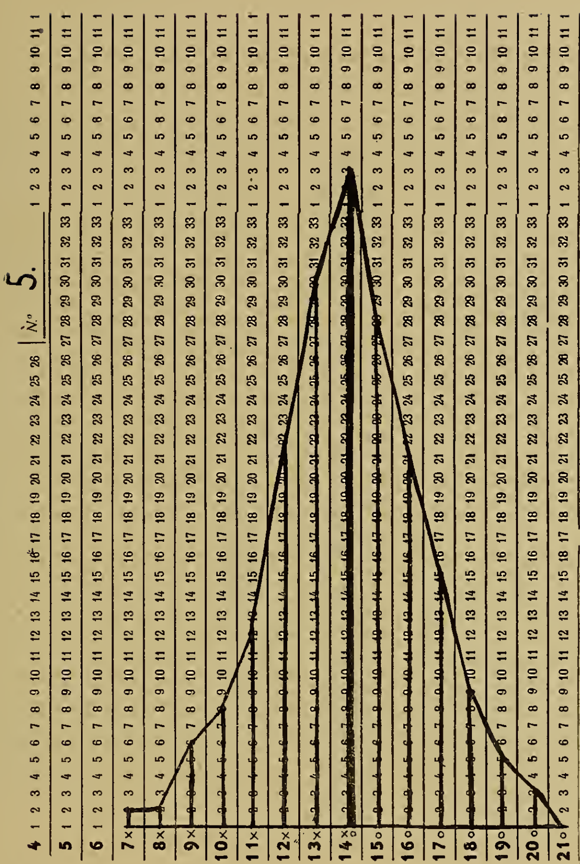

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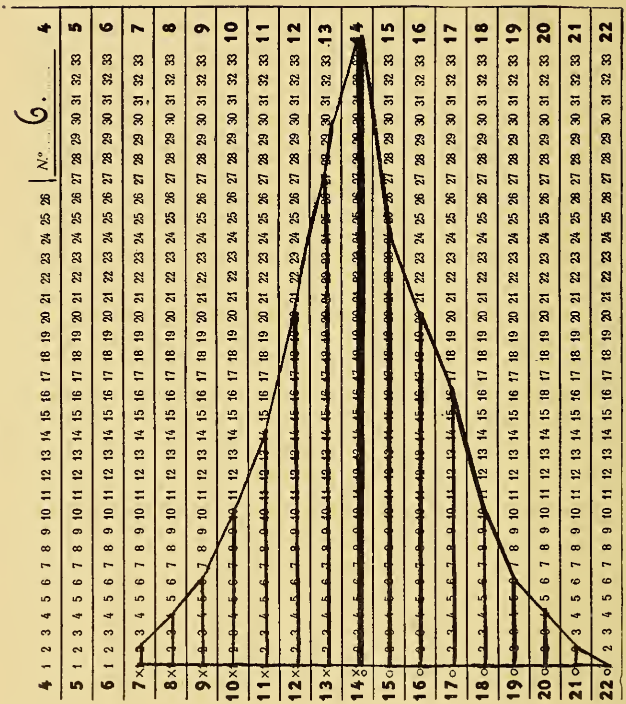

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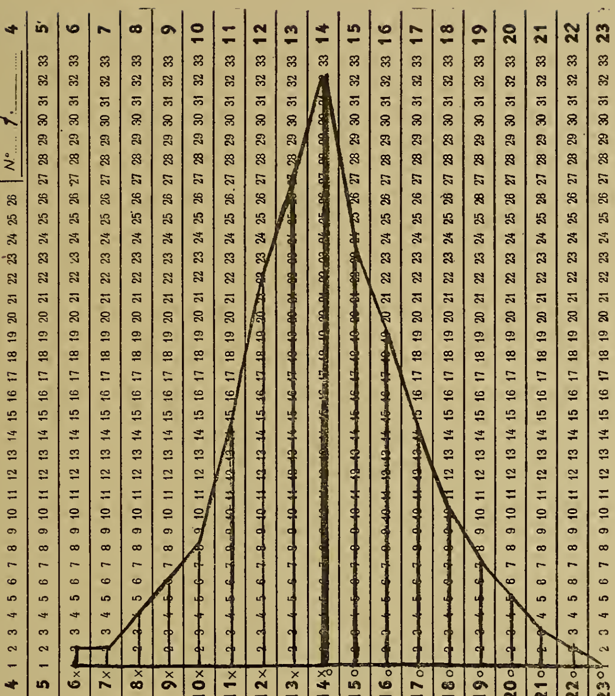

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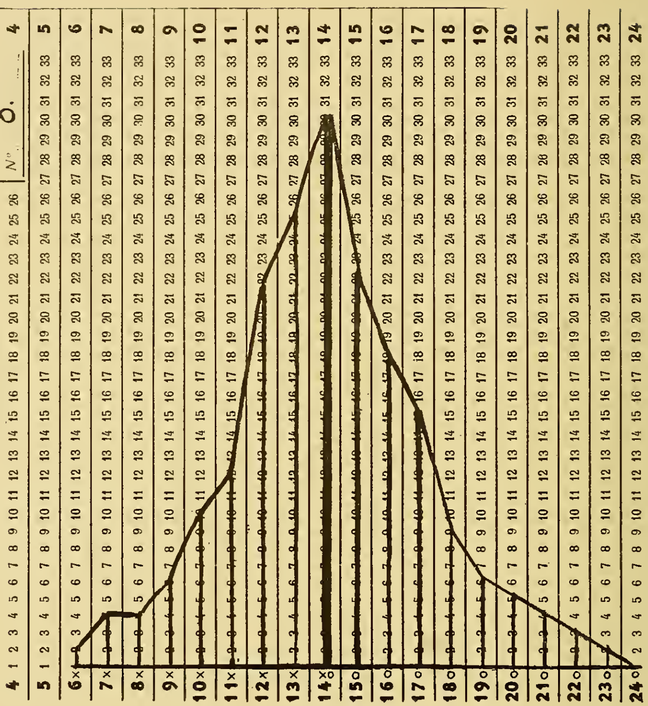


DIAGRAM 9.

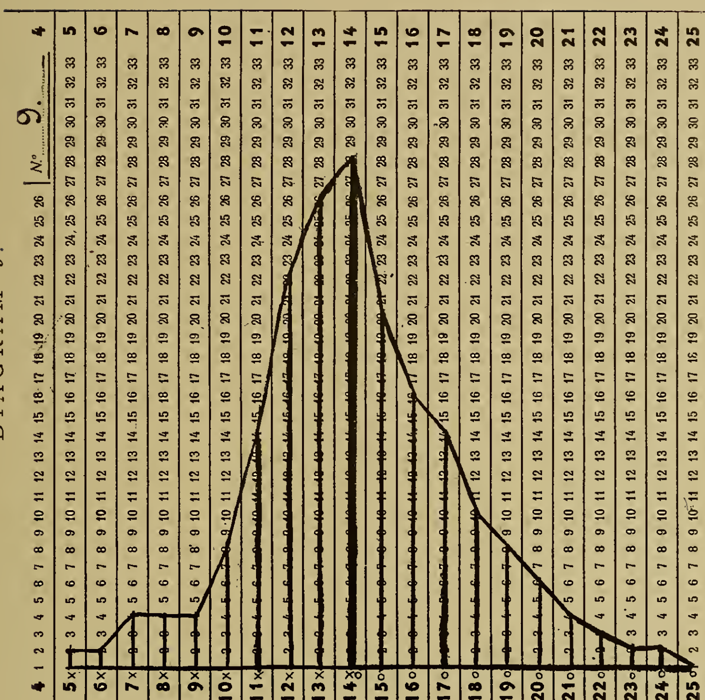


DIAGRAM 10.

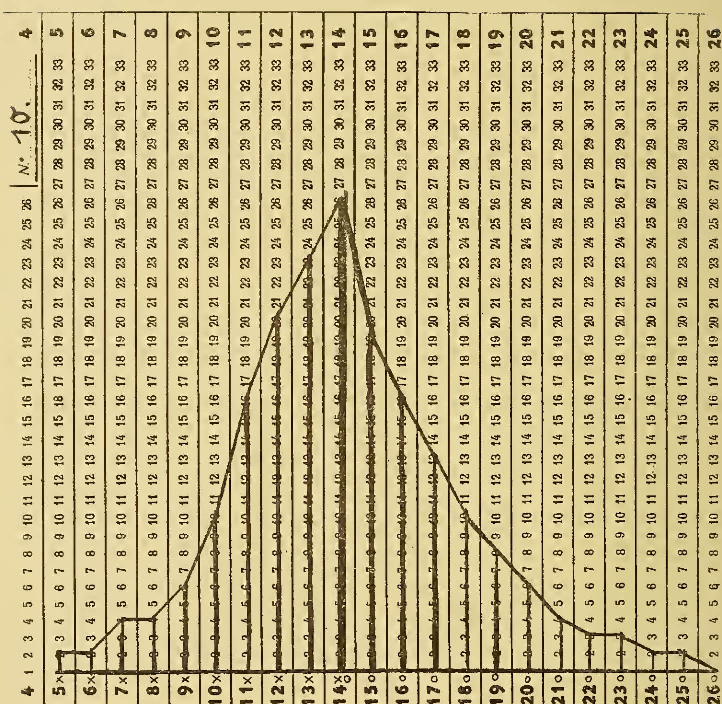
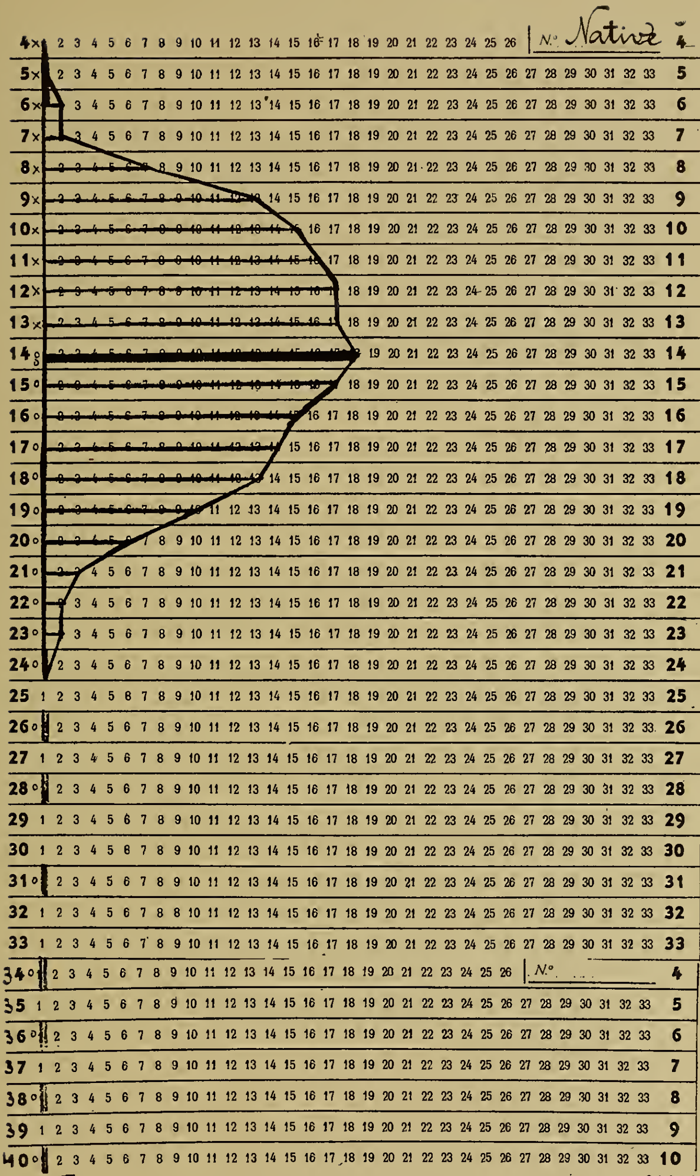


DIAGRAM NATIVE.



Chapter IV

FINE THREADS

In the foregoing chapter we have considered regularity chiefly with regard to the surface of the tissue, but regularity also includes fine threads, as defects, as they form a great impediment to speedy production and therefore require special examination.

Let us call in again the twelve experts of the second chapter and ask them: At which point a thread may be called a "fine thread?" Is size 10 a fine thread? Or size 9? Or 6? This time the twelve answers will be more uniform, if not more satisfactory: "We do not know."

In fact, the answer to this question is not quite easy, and in order to find it, it is necessary to contemplate the work as well of the reeler as of the weaver.

Let us begin with the latter. The loom evidently exercises nearly the same tension "T" on the thread,

whether it is working with size 9/11 or 29/31. It remains in both cases the same instrument with nearly identical qualities, and of this follows that "T" represents an absolute and not a relative value, that is to say, that the loom requires a certain minimum resistance of the thread, without regard to its size. This tension can be measured by inserting a sensitive dynamometer into a well-mounted warp; with results at about 25 grams for the weaving-loom, and near to 35 grams for the lace-loom. The sizes 6.75 on the one hand, and 10 on the other, are able to resist this tension —as will be demonstrated in the chapter on Tensile Strength—and so we might establish, that for the weaving-loom the sizes below 6.75, for the lace-loom those below 10 are to be considered as "fine threads." This is in accordance with practical experience. The thinnest size generally used for single weaving is 11/13 (of which we know, however, that very often it is 12/13 to 12/14) and in this size it requires no extraordinary skill of the reeling-girl not to go below 6 3/4. There are reelers, however, who produce 10/12 and even 9/11 fit for single weaving; but it has not been possible till now to produce 8/10 for this purpose. In fact, the frequent thinning of 30 per cent. that was demonstrated as being inevitable in the last chapter, amounts to 2.7 den. for size 9; consequently every bale of 8/10 must needs contain a great many passages of size $(9 - 2.7) = 6.3$, which will break on the loom. While for 9/11 the size of inevitable fine threads will be $10 - 3 = 7$,

and therefore it is possible to produce 9/11 fit for single weaving.

The same way for size 14/15, that is generally employed for Malines, the inevitable thinning arrives at size 10.15, which is able to resist to the tension of 35 grammes required by the lace-loom.

But a conscientious testing establishment cannot top at these figures and consider its task accomplished by them, nor should it declare that a bale of 28/30 contains no fine threads, because the thinnest sizes occurring in it are above 6 3/4.

It was necessary, therefore, to establish relative measures, viz., such in proportion with the size of the tested bale, without regard to the fact that the loom is heedless of this size.

Theoretical researches, as used in the last chapter, could give but a general idea about the variations of fine threads between good and bad silk; these could be found only by practical experience. It was necessary to establish, by the work of many years and by experiments on thousands of bales, the proportions between the sizes reeled of 4, 5, 6, etc., cocoons, and only after having ascertained by long experience that f. i., not one bale of 19/21 appeared that did not contain at least size 14; this size 14 could be established as the minimum for S° 1, for size 19/21. On the other hand we had found that the worst bales of 19/21 went down as far as size 5, but also this fact had to be confirmed repeatedly, until we could fix the S° 10 at size 5.

It was demonstrated in the last chapter how the fine threads are to be found, but we said also how cautious we must be in drawing conclusions, having to do with extremes. Therefore we must not judge by the latter alone but must take into consideration the whole character of the triangle, and go on with our researches if the extreme found is not in accordance with the former. Also in this case experience is a great help, and difficulties that seemed puzzling at the beginning solve themselves readily later on.

By our calculations we arrived at the following formula:

$$D = \frac{\frac{3\,S}{4} - M}{\frac{S}{2}}$$

in which

D = Degree Serivalor,
M = Finest size of the tested bale,
S = Average size of the tested bale.

The following table may serve for practical purposes:

Not to be used a S°
of fine threads inferior to:

1.5	for	single	warps	9/11
2.5	"	"	"	10/12
3.5	"	"	"	11/13
4.5	"	"	"	12/14
5.0	"	"	"	13/15 and thicker.

As regards the methods by which till now fine threads were found, there exists none, if the counting of breaks at the winding should not be considered as an endeavor in this direction. Its uselessness will be demonstrated in the next chapter.

Chapter V

THE WINDING

THE testing method which we explained in the last chapter is an indirect and therefore not an ideal one. What we found out was the size of the weakest spots, what we wanted to know was how resistent or how little resistent this weakest spot will be. From the size we concluded to the resistance, and in a rather reliable way, but the direct way is better than the indirect one, and to test resistance itself would be no doubt preferable. There exist instruments for this purpose, called dynamometers, of which we shall speak later in the chapter on "Tensile Strength." Not only are these instruments and the whole method of using them unsuitable, as will be proved, but they can furnish only the average tensile strength, which is quite useless, as we may see from this example:

A chain A consists of 3 links that, tested individually, showed the tensile strengths of 102, 82 and 20 lb.; a chain B, equally of 3 links, showed the strengths of 32, 30 and 28 lb. Although the average

strength of A is 68 lb., that of B only 30 lb., A will break at a tension of 20 lb., while B will support up to 27 lbs., that is to say, the strength of B is in reality by 40 per cent. greater than that of A, though the *average* strength of the latter is more than twice that of the former.

Applying, then, the law of mechanics: "A chain has the strength of its weakest links" to silk, we can say: *"A warp has the strength of its weakest spots,"* and therefore it is this latter and not its average strength that we have to find out. There exist instruments also for this purpose, so called "continual dynamometers," but they have not proved reliable as yet, and moreover they are working very slowly, so that the testing of 80 kilometers, necessary for this purpose, would require 9 days of 10 working hours each.

But it appears that well performed winding could furnish an excellent way of determining the weak spots of great lengths and an indistinct comprehension of this fact might have contributed towards giving undue importance to the winding. For in order to perform it exactly, it would be an indispensable condition that the tension of the thread should be the same throughout—a condition that neither the official institutions nor the Laboratory Serivalor are able to accomplish fully. The difficulties are:

1. *The revolving speed of the supplying reel ought to be unvariable during the whole operation.* This

is impossible, as can readily be seen on an inserted dynamometer, which will show an average tension of 30 grams, f. i., by variations from 10 to 50 grams.

2. *The silk ought to lie on the reel in loose—not stuck together—threads.* But we know that the threads of every skein not "rereeled" are sticking together. The Conditioning Houses, and also the Laboratory Serivalor, arrive at a nearly satisfactory result by "rubbing off" the hard passages, that is to say, solving them by the "dry" way. The often employed "wet" way is quite wrong, and even prejudicial as will be demonstrated in another chapter.

3. *The length tested must be at least* 80 *kilometers, taken from* 20 *skeins* (4 kilometers of each), if the result obtained should be "constant." This is accomplished only by the L. S. but not by the Condiditioning Houses, for the latter employ 30 kilometers, in some places by 6 km. from 5 skeins, in others by 3 km. from 10 skeins, arriving at uncertain and contradictory results, as they acknowledge in the publication referred to in the first chapter.

4. *The tension during the winding must be proportional to the size of the thread tested.* This is not accomplished by the Conditioning Houses. They test all sizes by the same tension, about 4 grams, which is absolutely insufficient. A common *single cocoon-thread* will resist in different places tensions of 8 to 18 grams, and the silk thread consists of 4 to

8 and more cocoon threads! A tension of 4 grams will, therefore, make appear only those threads that lay broken in the skein, and those *stuck together* threads that were broken by the revolving force of the reel, although they may not even have been weak spots.

On the contrary, the Laboratory Serivalor is accomplishing this task by testing each size under a tension proportional to its average tensile strength. In this way it finds out (with the restrictions deriving from par, 1, etc.) the number of spots that, in a length of 80 kilometers, are thinner than 25 per cent. of the average size, the results being "constant."

This is not the place for entering into the details of construction of the apparatus by which the tension can be regulated according to the size. We confine ourselves to saying that by changing the speed and by cautiously employing "rope-friction" every variation may be obtained.

After having got so far, the greatest difficulties were overcome and it was easy to establish the extreme values of:

$$\left.\begin{array}{l}\text{B for the best silk (S}^\circ\text{ 1)}\\ \text{and}\\ \text{W for the worst silk (S}^\circ\text{ 10)}\end{array}\right\}\begin{array}{l}\text{expressed in breaks}\\ \text{within 80 kilometers.}\end{array}$$

D being the Degree Serivalor, C the number of breaks in 80 kilometers, N the number of degrees desired, we have the formula—

$$D = \frac{\dfrac{C}{W - B}}{N - 1}$$

This classification, together with that of Fine Threads (Chapter IV.) furnishes a reliable indication about the quantity of really weak spots—that is to say, possessing a tensile strength of less than 25 per cent. of that of the average size—contained in the bale tested.

The classification of "Winding" must not be considered, however, as more than it is: an indication of the speed allowed for rational winding with 36-40 reels to each girl, keeping reels and girl continually employed. The S° 1 indicates that the bale may be wound with the speed of meters in the minute, equal to twelve times its size; therefore size 13/15 with 12 x 14 = 168 meters. The other degrees indicate:

S°	2,	3,	4,	5,	6,	7,	8,	9,	10.
Size multiplied by	11	10	9	8	7	6	5	4	3
that is, for 13/15 meters:	154	140	126	112	98	84	70	56	42

But the winding does not allow any conclusions to the other qualities of the thread. That this erroneous conclusion is pretty general, is in consequence of the fact that not very long ago the weaver was employing only thrown silks for whose quality he accepted the throwster's judgment; and for the throwster, of course, the winding forms a very important

part of his work. Another reason is that winding is the operation whose results are first known to the manufacturer, and this caused the false belief that these results are characteristic of the whole bale. But size 18/20, f. i., is always winding well—does this signify that all bales of this size are of the same quality?

Japans in general are better winders than many Italians. Are they better for that? China Double Extra winding worse than Italians—are they really inferior? Besides, is the judgment of the winding-girls constant? Anyone who will make experiments in this regard will find that they are contradictory to each other, and that the same girl will judge differently of the same bale to-day and to-morrow (of course being ignorant of the fact that it was the same bale).

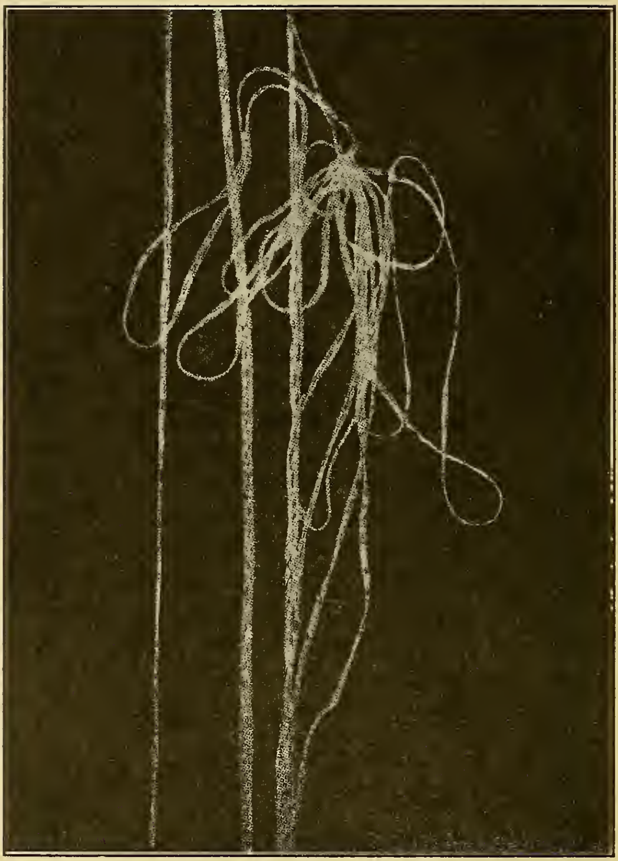
The exact test for winding is important for the throwster; for the manufacturer it is but an interesting detail, without great value for recognizing the general quality of the bale tested.

It is true that it is the duty of a good reeler to furnish silk that winds well, as already expressed by the tenth rule of Chapter II. The means to arrive at this end are not generally known, and it is not our object to explain them here. The Japanese have adopted the most radical one: Rereeling, which, however, is too expensive in Europe. Also the "rubbing off" of the hard passages is practiced with good success by many reelers. Some also try to hinder the sticking to-

gether of the threads by applying diluted greasy substances to them before they get on the reel. But this induces to "charging" the silk, and moreover brings about a certain mouldy odor, if not very cleverly done.

But it is a fact that there exist good winding silks which are neither rereeled nor greased, nay, that these are the natural product, if the reeler knows his business. This is sufficient, and those who are producing badly winding silks must bear the consequences.

One of the consequences of the sticking together of threads is the Double Ends which occur so often, and which the reeler does not know how to avoid. In fact, they are rarely the latter's fault, as everybody will acknowledge who is acquainted with the work of reeling. The double ends are formed themselves during the winding by the stronger thread tearing away the weaker one at the spot where they stick together, and drag it along for a length of time, until another break interrupts the double thread formed in this way. (Such passages turned up as extremely heavy ones in the 200 skeins that we tested for Regularity.) Of the same origin are the "underslipped ends" (ends covered by the running thread on the bobbin) which so often trouble the winder.



Microphotograph of a "Flock."

Chapter VI

FLOCKS

After having thus far examined those defects that are measurable by their length, we arrive at those that are measurable no more, but become visible and therefore numerable. They appear as knobs and knots in the thread, which however do not consist, as many believe, of an adherent alien material (waste) but of a normal cocoon-thread that fell off the cocoon in many loops at once and had no time for stretching itself before it was united to the main thread. The opposite photograph (taken from a publication of the Laboratory of the Stagionatura Anonima, Milano, with kind permission of the editors) gives a good picture of one of these "flocks."

The origin of this defect was explained in the fifth, sixth, seventh and eighth of the reeling-rules, and we might say at once that it cannot be completely

avoided. Good reelers, of course, produce a cleaner thread than careless ones, but it is impossible to produce a thread as clean as it is required by the loom. The flocks must be removed, therefore, from the skeins by special "cleaning-girls," and this is a wearisome task that does not give satisfactory results and, moreover, very often is the cause of the skeins "falling in layers." The real cleaning must be done by well performed warping. This should be done:

1. With not more than 300 threads at once;

2. By one clever working girl and an assistant;

3. The mounting should be such that the working girls can overlook a length of two yards, and that between the single threads there remains a distance of about ¼ inch.

4. The speed must be regulated in order that the working girls may have time for removing the flock and reknotting the thread, without stopping the machine. (Such stoppings produce stripes of different tension in the warp, which in the tissue appear of different luster.)

Only such silk ought to be declared as not clean whose number of flocks is too great for allowing their removal without stopping the machine. All others will yield, by this procedure, a cleaner warp than can be produced by the best reeler, and the costs of

the cleaning are slight, even in America. By adopting this system, the manufacturer has the possibility of being more indulgent with regard to cleanliness and consequently buying at a cheaper price.

The L. S. calls such defects "flocks" when they increase the diameter of the thread, distinguishing however between small ones, that is, those that pass a reed of 58 splits, of No. 94, to the centimeter—157 to the Paris inch, and bigger ones—viz., those of a diamèter below or above 1/10 millimeter.

They are found by passing the 20 skeins over a dark background and their number is brought into proportion to the kilogram. The extremes for B (best) and W (worst) have been established empirically, but it appeared unfeasible to establish the gradation by arithmetic division of the series; it was necessary to recur to geometrical progression, according to the following formula:

a, b, c, d, e, ... being the components of the series, and $y + z$ the difference between a and b, then

$$c = b + y + 2z$$
$$d = c + y + 3z$$
$$e = d + y + 4z, \text{ etc.}$$

There does not exist an official system of judging cleanliness as yet. The silk inspecteurs, however, give their attention chiefly to this quality, accepting "clean" bales as good ones, and "unclean" bales as bad ones. By this they are committing the logical error

of concluding from the parts to the whole, instead of the opposite way. It is the same as if from the fact that every bird has two legs I would conclude that a man standing before me must be a bird, because he has two legs. The inspecteurs, however, reason in this way, thinking good silk is clean, consequently clean silk must be good.

Cleanliness is one of the qualities of good silk; it may be considered of more or less importance by one or the other, but in no case is it considered as Quality itself. We have also seen that it can be improved by subsequent procedures while *as Quality can be considered only those intrinsic characteristics that are unalterable.*

As practical hints for the manufacturer we might add that according to our experience the S° 3 of cleanliness is sufficient nearly for all reasonable claims; but it is not too difficult to find S° 2 and even 1½. On the other hand even S° 5 might be improved, in the way explained before, to S° 1, with slight costs.

CHAPTER VII

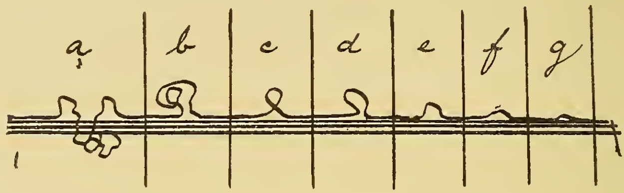
LOOPS

OUR way through the different forms of the original defects of silk, which, as our readers will have noticed, leads from the greater to the smaller, has now arrived at those that are hardly visible and therefore difficult to find. Nevertheless it is necessary to find and control them, for just as the bacilli to mankind, these little defects become dangerous to the loom by their enormous number.

The defect which is the object of the present chapter is but a smaller form of the "flocks" treated in the preceding one, but it occurs on the average 5,000 times as often, and therefore makes itself very much felt.

When the roundabout way made by the "bava," before uniting itself with its neighbors, is not great enough to render the thread *sensibly* thicker, we call it "loop." The following schematic design shows under *a, b, c, d, e, f, g,* the transitions from flocks to

loops; the gradations are countless, but the line of demarcation is given, as we said before, by the *sensible* thickening of the diameter.



As the loops do not *sensibly* increase the diameter of the thread, they pass unnoticed also through the finest reed, and in this regard the weaver need not pay any attention to them. But while being closely pressed together on the warp-beam they get entangled with their neighboring threads and hinder the clear opening of the shed, which is the cause of many breaks and weaving-defects; the weaver therefore justly fears the dangerous little enemy. Its origin is heedlessness with regard to the Seventh of the reeling rules, the complete observation of which, however, is not possible. The worm crosses the thread in infinite variations of density, and it might be said that in this regard not one cocoon is quite the same as another; consequently the assorting according to the texture is possible only to a certain degree, that is to say, the defect of loops is inevitable. In fact even the best silk contains about 50,000 of them to the kilo, the

worst, however, more than a hundred times as many, that is about five millions.

The cocoons of widest texture are called in Italy "Bombaggiati," an inappropriate term, because of not expressing the real thing; more suitable is the French "Satinés" as the wider texture causes greater luster on the cocoon as well as on the tissue.

This, our theory of the origin of loops, is unknown to the reelers and is published here for the first time. We must not forget that the whole procedure in spinning has not got very far yet beyond the simple principles of a rustic home industry and avails itself very little of scientific methods.

That the "Bombaggiati" must be eliminated is known nearly to all reelers—which does not imply that they are all doing it—but when I tried to find out what "Bombaggiati" really are, nobody could give me a precise definition, as they are not judged by clearly visible marks, but found out by an uncertain, instinctive distinction. How reliable this way is may be ascertained by the following experiments: Give an order to a very clever "sorting-woman" to eliminate all "Bombaggiati" out of a basket of unassorted cocoons; after a while she will bring back the basket of "purified" cocoons, and we put it aside. The next day we give the same cocoons to the same woman in another basket, and she will find more "Bombaggiati" among them, and so we might repeat the thing five times and always new "Bombaggiati" will turn up.

Or we take ten cocoons of middle texture (they are quickly and surely discernible under the microscope) and show them to ten experts with the question whether they are "Bombaggiati" or not; half of them will answer in the affirmative, the other half in the negative, and none will recognize the real thing: that the cocoons hold the middle between the good and the bad ones in this respect.

Even real "Bombaggiati," however, furnish a good thread, if they are not mixed together with others, and are treated according to their nature; for the procedure adapted to them is not fit for cocoons of denser texture.

An official method of testing exists as little for "loops" as for "flocks." Inspectors think they can judge of the defect by straightening the skein so that it forms an even, glossy surface, while looking at it in a corner with their backs to the window, so that their eyes receive the light reflected by the silk. In this way they can examine the surface only, which of course is insufficient, as the inside of the skein remains unknown and, moreover, they are deceived by the circumstance that the loops become the more visible the more lustrous the silk is. And as it is the task of the good reeler to produce a thread as lustrous as possible, they will see more loops in good silk than in inferior silk. I myself produced once, by a new system, a skein of extraordinary luster, but all the reeling-girls, the forewoman and the director of the establishment declared

it to be very "downy," that is to say, full of "loops." Tested in my laboratory it proved to contain only 20,000 loops to the kilo, that is, better than S° 1.

The loops must not be looked for in the skein but on the single thread, where they can be counted, which, however, is not an easy task. The generally used "black" boards, reels, etc., are not black in an optical sense, but dark blue, or green, or brown and very fatiguing to the eye. Only exact optical contrivances allow continual and easy working and clear discerning and counting.

Especially do the many "casts," viz., beginnings of new cocoon-threads, lead to errors, as they are generally accompanied by loops, which however must not be considered as a defect. Dark days and artificial light are unsuitable for the work, and in the Winter, when there are only a few hours of good daylight, these must be well used.

The calculation of the ten degrees Serivalor is done according to the formula given in the last chapter, but it is to be observed that the visibility of loops is proportional to the square root of the thicker size, by reasons known to geometry (diameter of cylinders).

As to practical use, our experiences have proved that the manufacturer may employ even S° 4, but it is safer not to go beyond 3½. Of course, the claims are different according to the articles manufactured and to the customers. It is interesting in this regard to consider the relation of Cevennes silks to their customers.

These silks are known as very "duveteuses," but people simply say, shrugging their shoulders, *"C'est la nature de ces soies,"* which, however, is only partly true. In the nature of these silks lies only a small part of the cause, viz., their thick and hard gum, which, as said in the Fifth spinning-rule, opposes resistance to the straightening of the thread. The chief cause lies in the fact that French reelers with their system *"à la Chambon"* arrive at too scarce a production and in order to make up as well as possible for this drawback are obliged to a forced speed (about 180 meters in the minute, instead of 110-130 meters with the system *"à la tavelle"*) which does not allow the necessary time for the right soaking and straightening of the thread (see Fourth, Fifth and Eighth spinning-rules). But with the system *"à la tavelle"* of Cevennes-cocoons there could be reeled a thread that would get S° 1-2 for "loops."

Nevertheless Cevennes silks, as they are, have their faithful friends, who even pay good prices for them. This seems to contradict the assertion that "loops" are a serious defect; in reality it only proves the truth of what we said in the Prospectus: that every silk is good, if employed in the right way.

Silks reeled *"à la Chambon"* have not only their defects but also their advantages, and such that are hardly to be obtained by reeling *"à la tavelle"*; they have nearly no weak spots and this makes them especially fit for the lace-loom. In fact, Lyon manu-

factures employ them chiefly for "tulles" and similar purposes, viz., for light warps, that are not liable to entangle. The Lyon industry has always shown an extraordinary ability in employing every kind of silk according to its special qualities—an ability acquired by long and intelligent observation and preserved by tradition. But we would assert that the same advantage may be obtained in a short time by making use of the classifications of "Serivalor."

Chapter VIII

COHESION

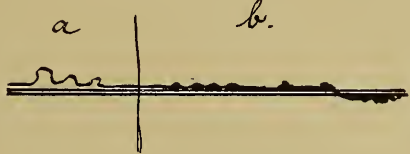
Let us again cast a glance at the various forms of the original defects, thus far treated upon. From those that are measurable we have proceeded to those that can be measured no more, but can still be seen and felt, and thence to those that are hardly visible but still countable. Now we are arrived at those which are neither measurable nor visible nor countable, to avoid which is as difficult for the reeler as it is difficult for the observer to find them, and which cannot be improved.

From their general diffusion and from the impossibility of improving them there results that they are reflecting *the intrinsic constitution of* the silk and consequently its real "Quality," while all the other forms are only accessories.

An intermediate form between "loops" and the invisible defect of bad cohesion are the so-called "rognose" (scabious) threads, which the reelers sup-

pose come from cocoons which though of normal appearance, yield a thread affected by a disease. In reality this defect is nothing but the smallest form of "loops" caused by not observing the Fourth, Fifth, Seventh and Eighth of the spinning rules; it forms a visible loop no more, but apparently compact thickenings of the thread, which, however, under a lens of three-fold linear enlargement are recognizable as small loops, or rather undulations.

The following schematic design shows the difference between *a* "loops" and *b* "rognose":



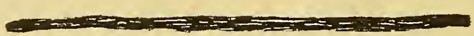
Such threads are discernible also to the naked eye by a certain rough appearance. This defect, although a serious one, does not occur frequently. In the testing of the Laboratory Serivalor it is included under "Cohesion."

Let us now suppose that the reeler has succeeded in producing a thread that appears straight and glossy not only to the naked eye, but also under enlargements of 5, 10 and even 20 diameters. Only when employing one of 30 diameters, do we begin to see that the straightness is only apparent; and, increasing the

enlargement, we recognize that there does not exist a cocoon-thread completely straight and united with its neighbors without any gap between them, and we become aware of the fact that the loom accepts as the best silks those that contain the smallest gaps, while those containing large gaps turn out to work badly on it. This makes it evident that this deeply concealed, perpetually acting constitution of every thousandth of an inch forms the real "quality" of the silk thread.

This is the reason why one warp gets roughened by the reed and the other is not, why in the one so many threads are split (which then are mistaken as double ends), while in the other they remain like wires, why one tram or organzine comes back clean and faultless from the dyer's, while others get downy and swollen. (The latter defect must be distinguished from "lousiness," which will be treated separately.)

That also this defect is but another want of "uniformity" is shown by the fact that even these smallest undulations alter the diameter of the thread, as may be seen from the following schematic design:



Why, then, is this quality of such prevailing influence on the loom? Because it makes the thread more or less resistant against *friction,* and because it is chiefly friction and not tension, as is the general

belief, that the loom exercises on the thread. In the same way all products of the textile industry, clothes, linen, etc., are worn out by friction.

This made me, in 1904, give the name of "Friction" to this quality of the silk thread, and shortly afterwards this term, unknown till then in the silk trade, was beginning to turn up in many places. Meanwhile I had found that I had misnamed the thing, that friction might be considered as the way of testing silk in this regard, but that the quality itself must be rightly called "Cohesion." Thereafter I used this latter term in occasional publications, and the consequence was that it completely replaced the other. It is now generally used by people who test the thread with their fingernail (using a piece of about 1/500 of the minimum length required for a reliable result), but who will never admit of whom they borrowed the notion and the term. For an European silk man knows everything by himself and will never acknowledge that he has been taught by anybody, and prize competitions for new methods of testing silk cannot be hoped for in Europe.

I have the satisfaction of knowing that the Austrian Government, which previously had tested army-cloth by the usual dynamometers, after 1904 adopted the testing by friction, which I had been the first to recommend.

Whether a silk thread is more or less perfect in

cohesion depends on the observation of the spinning rules from four to nine, especially of the last one, which, however, can hardly be controlled.

Whether the "twist" is right, only an experienced observer can see only from a certain visual angle, and the forewoman in nine cases out of ten has no idea of the thing. All twists break and must be renewed; suppose this happens to every reeling-girl two to four times in the hour, every bale contains 5,000 to 10,000 different twists, viz., as many different cohesions, and how should the manager, or the reeler, know how all these have succeeded?

A decisive influence is also exercised by the water used in the reeling, which, however, does not remain the same throughout the year, but is altered in its composition by dry periods on the one hand, and heavy rains on the other, and accordingly dissolves the sericin more or less. (Chemical corrections have proved useless till now, according to my experience.) Of course, it would be the best thing to employ distilled water, if it were not too expensive in Italy, which has no cheap coal; the cost would amount to one lire per kilo—about nine cents per pound, and Italian reelers do not gain enough to be able to support this difference, as we shall see in the Fourteenth and Fifteenth chapters.

Finally also, the nature of the sericin itself is of great influence, and this varies according to the race, and to the region where the latter is reared. Though

very little is known in this regard, it might be said in general that hill countries of about 300 to 400 meters above the level of the sea yield the best cocoons in Italy; that the race called *"Gialli puri"* is the best, and next to it in downward gradation: *"Incroci Chinesi," "Bigialli," "Incroci Giapponesi."*

Though generally recognized as the best race, the rearing of *"Gialli puri"* is diminishing instead of increasing, owing to the circumstance that an ounce of their seed yields 40 to 50 kilos of cocoons only, while an ounce of "Incrocichinesi" may yield 70 to 90 kilos. The cocoons of the former ought to bring 50 per cent. more in consequence, but the reeler pays only 14 per cent. more, as it is impossible also for him to obtain a higher advance from his customers.

We have already explained the influence of cohesion on the loom. Throwsters never become aware of this influence and remain ignorant of the causes. Even the real "tensile strength" remains unknown to them; what they find out is only the more or less frequent occurring of weak spots (that is, appearing weak only on their machines), and in this regard they are as competent as it is possible for empirics to be.

We said before that the quality of cohesion is visible under the microscope; but from visibility to measuring and classifying the defects is a long and wearisome way.

Some direction is given by the fact that the num-

ber and the size of the gaps increase with the enlargement; it would be possible therefore to establish a gradation according to their visibility under various lenses. But here arises the difficulty that the range of vision of a microscope diminishes proportionately to its enlargement, while on the other hand great lengths must be examined in order to arrive at constant results. It is necessary therefore to choose microscopes of relatively great range and to find a method of looking over great lengths in the shortest time possible. But this method renders photographic reproduction impossible, as the microscope must be kept moving on the object. The various forms found in this way cannot be represented, therefore, by description or illustration, but only by demonstration of the objects, let us say, in a lecture.

Another suggestion is furnished by the conclusion that the best silk must be the one that has the smallest diameter in relation to its size, the straight line being the shortest. But it is difficult to find out the diameter of a silk thread, not only because it is of a soft material yielding to pressure, but also because the diameter varies according to the number of cocoons employed, as will be explained in another chapter. This makes the task very complicated and not profitable.

A third way, and evidently the best of all, is the direct measuring of the friction necessary for wearing out the thread. But there does not yet exist a

reliable apparatus for measuring friction, and science in general is not very much advanced in this direction and does not offer any help.

For two properties are acting here, which, though of opposite nature, are always coupled together—Friction and Adhesion. The more smooth the surface of a thing is, the less is the friction developed by it, and, vice versa, the larger the adhesion. I have not found an apparatus yet, except the one constructed by myself, that separates these two influences—nay, even none that would have betrayed a notion of their existence.

The Austrian Government tries to overcome this difficulty by applying acids to the tissues to be tested, in order to render their surface rough—a doubtful procedure which I would not imitate.

Having succeeded, as said before, in constructing an apparatus that yields reliable and constant results, the transformation of these into Degrees Serivalor offered no difficulties. It would be useless to give the formula here, as the components cannot be found without the apparatus.

All S^o of Cohesion may be employed for double-width weaving, even with great speed, but the reed must be the coarser, the worse the S^o is.

This is illustrated by the following table:

Degree of Cohesion:	1	2	3	4	5	6	7	8	9	10
Threads to the split.	Splits to the centimeter.									
1							64	50	37	24
2				64	56	49	41	34	26	19
3	62	57	52	47	42	37	32	27	22	17
4	50	47	43	39	35	31	27	23	19	15
5	42	38	35	32	29	26	23	20	17	14
6	38	35	32	29	27	24	21	19	16	13
7	32	30	28	25	23	21	18	16	14	12
8	30	28	26	24	22	20	18	16	14	12

Of the proportion of splits to the centimeter and to the Paris inch, as well as of the right size of the splits, we shall speak in a later chapter.

Empirics always had the indistinct feeling that there was something in the quality of silk which they could not find out by their simple means. They therefore had recourse to the dynamometer by the aid of which it is possible to judge of tensile strength and ductility, if it is cautiously employed. This, however, was not done, ductility was misnamed elasticity, and so people arrived at delusive results, as we shall see in the following chapters.

Chapter IX

DUCTILITY AND ELASTICITY

JUST as the two qualities mentioned in the last chapter, friction and adhesion, though of opposite nature, are often confounded with each other, it is also the same with regard to ductility and elasticity, and for similar reasons.

If a solid body is exposed to a certain tension it is extending in that direction. *All solid bodies are extensible,* but of course they are not all so in the same degree, as can easily be seen by comparing the ductility of a hemp rope with that of a copper wire and that of an elastic string. If the tension is interrupted before it arrives at breaking the body, the latter returns more or less to its former length, in consequence of the

quality called *elasticity*. In this regard India rubber forms no exception; it is exceptional only by its extraordinary *ductility* in connection with its elasticity. On the contrary there exists a small number of bodies possessing considerable ductility with very little elasticity and to these belong all hardened, slimy substances, and among these also the silk thread.

Considering now various bodies with regard to their ductility and elasticity, f. i., steel, copper, India rubber, cotton, silk, etc., we soon find out that they are in general the stronger, and the more resistant the more they possess of elasticity *and the less of ductility*, and vice versa. The *silk thread, however, possesses only great ductility and nearly no elasticity,* and trying to measure the former quality and considering it a great advantage forms a fatal error, which is not improved by misnaming as "elasticity" what is only ductility.

Certainly, it would be desirable to find elastic silk, and consequently to test the thread with regard to this quality. Till now, however, nothing has been tried this way, and should it ever be done it would certainly appear that the more elastic silks are the less tensible ones, and vice versa.

Constantly occurring and nevertheless very little observed proof of this fact are the lustrous stripes in the warp, of which we said in the sixth chapter that they are caused by the stopping of the warping machine. The slightly increased tension caused by the

new impulse given to the bobbins is sufficient for lengthening the threads, and the more so the more ductility, that is to say, *the less cohesion they possess.* If the silk threads were elastic they would return to their former length after this slight tension; by their higher luster, visible afterwards in the tissue, they show that this is not the case, except when, as said above, they are of good cohesion, in which case the defect becomes imperceptible.

Doughy bodies like "macaroni," which are also a hardened, slimy substance, are comparatively resistant and nearly not tensible when very dry; but when soaked very tensible and not resistant.

In which state are they stronger?

We can observe a similar thing with silk. Cutting open a sizing skein, we divide the 400 threads into two equal parts, one of which we bring into air of 100 per cent. humidity, leaving it there for twenty-four hours, while we expose the other to a temperature of about 80° C. in order to desiccate. Then we try 100 threads of each half on the dynamometer (taking out only one thread at a time and leaving the others in their actual condition), and the result will be, that the wet threads are more tensible, *but less resistant,* the dry ones less tensible and more resistant.

Which is the better of the two?

In the following chapter we shall demonstrate how

cautiously the results of the dynamometer are to be made use of.

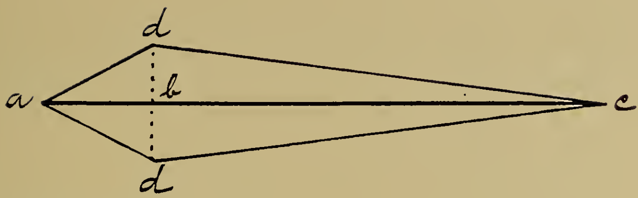
Pasting two sheets of paper together, it will be an easy thing to draw the one along the other as long as the gum is wet, while they will oppose strong resistance, as soon as the gum has become dry; we see that the molecules of the gum were easily gliding when wet, but strongly adherent to each other when dry. Similarly, the molecules of the wet silk thread will glide along each other, which makes the thread tensible, but much less resistant than the dry one.

Elasticity and ductility are therefore connected opposite qualities (just as friction and adhesion); the former alone is an advantage, *the latter a defect.*

(The difficulties arising from dry air in the weaving-rooms are caused by electric currents, which are accumulated by dry objects, and they have nothing to do with elasticity or ductility. We shall speak of this matter in the chapter on "Soaking.")

Turning now to the loom, we see, first of all, that it does not require any considerable ductility of the warp. By the opening of the shed the threads form the hypotenuses *a d* and *d c* of two rectangular triangles, whose other sides:

b d	measures	about	12	centimeters
a b	"	"	30	"
b c	"	"	120	"



Consequently:

$$a\,b + b\,c = 30 + 120 = 150 \text{ ctm.}$$

$$a\,d + d\,c = \sqrt{a\,b^2 + c\,d^2} + \sqrt{b\,c^2 + b\,d^2} = \sqrt{900 + 144} + \sqrt{14{,}400 + 144} = 32.3 + 120.6 = 152.9 \text{ ctm.}$$

that is to say, that the length of 150 ctm. must be increased by 2.9 ctm. = 2 per cent. only, which is nearly performed by the mere movement of the warp-beam, with very little tension of the warp.

In fact, the same length is yielded by the cotton warp that has only 2 to 4 per cent. of ductility and by Schappe, with 4 to 6 per cent., while silk has at least 16 per cent. of ductility. Weaving with a warp mixed of these three materials, we shall find that the silk threads show the greatest, and the cotton threads the smallest number of breaks, *in complete opposition to their ductility!*

That ductility is a defect rather than an advantage appears well known in other industries. I have seen price lists of steel-works accentuating the fact that the ductility of their steels *diminishes with their better quality.*

But experience has proved that yellow silks in general are more tensible and nevertheless they are of better quality than white ones, and I must explain this apparent contradiction to my theory.

The reasons for this phenomenon are:

a. Humidity is contained chiefly in the sericin and much less in the fibroine. Raw, yellow silks therefore contain, under the same conditions, more humidity than white ones, and consequently are more tensible. (In boiled off silks this difference does not exist, and they are also much less tensible.)

b. The tensibility is proportional to the diameter; irregular silks therefore show greater differences in tensibility. This latter quality would allow then to judge the regularity of the thread—but in such an uncertain and inconstant way, that it cannot be of any avail. We have explained in the third, fourth and fifth chapters the only reliable way of judging regularity.

Some manufacturers prefer tensible warps for the reason that these become longer in the finish and so appear profitable. This reasoning is absolutely wrong. The tissue becomes narrower by as much as it becomes longer, and so its surface contains the same number of square inches as before; but at the same time it becomes by 10-15 per cent. less lustrous and therefore less valuable. To every finisher ought to be given the strict order not to lengthen the tissue even by one inch, and if it is possible to find one, who fulfills this desire,

it will turn out that a not lengthened warp of organzine 17/19 yields a finer "satin tramé cotton" than a lengthened one with the same number of threads of size 19/21. *"Probatum est!"*

In consequence of the reasons expressed in this chapter, we have abandoned long ago the testing for ductility (which in 1904 we considered still important, like everybody else) and we presume that after some time we shall do the same with "tensile strength," which will be the object of the next chapter.

Chapter X

TENSILE STRENGTH

It is natural to test the consistency of a body by trying to separate its molecules, and to judge of its strength from the force necessary for this separation. But, of course, such a test can be decisive only if it is trying the material in the same way as the latter is tried when practically employed, and the testing of friction would be of as little avail for the cable of a cable-railway, as that of tension would be for a car-wheel.

Physics, therefore, does not speak of "strength" in general, but of resistance against pressure, friction, tension, etc., and carefully avoids concluding from one of these qualities to the other.

But this is what we are doing by testing the tensile

strength of silk, while on the loom it is exposed to strong friction, but next to no tension, and we make the thing worse by testing the wrong way.

Nobody will demand a thread of size nine that it should possess the same tensile strength as one of size eighteen of the same bale; but neither can we claim this of size 13.9 in comparison with 14.1. Consequently it is of no use to declare, as it is done now, that twenty threads drawn from a certain bale show an average tensile strength of fifty grams, *if the size of these twenty threads is not fixed* at the same time. Nor can we expect that by a kind hazard that these twenty threads will represent the average size of the bale, when we know that neither 200, nor 2,000, nor even 20,000 threads are sufficient, but that 90,000 meters of length are required for this purpose.

It is necessary, therefore, to size the tested threads, that is to say, to measure and weigh them exactly, and, as their weight is altered by humidity, to establish their *absolute* weight.

This can be done only by means of a highly sensitive balance mounted on a drying-stand. But, as the balance is influenced by the warm air rising from the latter, when the weight is very light, it is necessary to test a great number of threads, viz., some hundreds, better a thousand, of them, and this takes up very much time on the usual dynamometers.

In order to express the result of the testing by

one single number, the employed power is represented not by the weight, but *by length in kilometers,* and called "breaking length." The supposition is that the thread is let down into a great depth until it is broken by its own length; in reality, of course, the weight necessary for breaking it is transformed into length; the formula is:

$$B\,L = \frac{W}{\frac{10S}{9}}$$

in which B L = breaking length, W = weight, S = size.

However logical seems this method, it is far from expressing the real resistance of a material against all destructive influences.

We become aware of this fact by comparing the average breaking length of various threads with each other; it is for wool about five (kilometers); cotton, fifteen; flax, twenty; hemp, twenty-five; silk, thirty-five.

According to this, silk ought to be the most resistant of these materials. But as we know that it is far from being so, we recognize that there must be a logical error either in the definition itself or in the way it is ascertained.

We shall see that it is the case with both.

Still more unsuitable appears the establishing of the breaking lengths, if we compare those of textile threads with those of similar forms in other materials.

Steel wire, f. i., has a tensile strength of 120 kilograms to the transverse section of one millimeter square, and as its specific gravity is about eight, its breaking length is

$$\frac{12{,}000}{8 \times 1{,}000} = 15$$

that is to say, not even half that of the silk thread!

Nevertheless, everybody will justly consider a wire rope as infinitely safer for a cable-railway than a silk rope.

Considering, however, the three materials: silk, cotton, steel, with regard to their resistance against friction, we learn that steel surpasses the other two many thousand times, while cotton is still considerably more resistant than silk. By this we see that the testing of friction is by far a truer expression of the molecular consistency than the deceptive breaking length.

On the other hand, the testing of steel wires with regard to their tensile strength gives valuable results; why are they wrong only in our case?

On account of the ductility of the silk thread.

In testing tensible materials, the time of their exposure to tension is decisive, while the instruments actually in use, the dynamometers are completely regardless of time.

A steel wire that will break under a weight of

120 kilograms will resist for weeks a weight of 110 kilograms, *because it is nearly not tensible;* but a silk thread breaking under sixty grams will break also under thirty grams if to these thirty grams is allowed the time necessary for extending the thread. The sixty grams are wrongly indicated by the dynamometer, because it made the thirty grams increase too rapidly, and before we had time to recognize that they would have sufficed to break the thread.

But while the dynamometer makes a rapidly *increasing* weight act only *for seconds,* the loom will act the opposite way: it exercises a *slight* tension during *a long time* (several weeks) and every bit of the thread receives on its way from the warp-beam to the cloth-beam about 6,000 jerks, which, though each of them lasts less than one-half second, sum up to a considerable time: about an hour.

Seeing, then, how fundamental the difference is between the practical work and the testing instrument, we need not wonder that the indications of the latter are of no avail, even if the instrument itself be exact, which a pendulum-dynamometer cannot be, as the pendulum very often receives a swinging start at the decisive moment, which makes wrong results appear.

It is possible to correct these to a certain degree, but it requires a skillful and experienced hand to do so. The same with other difficulties, f. i., that any difference in humidity influences the tensile strength,

while it is impossible to bring the thread to a standard humidity. This must be overcome by calculating corrections for each occurring degree of humidity (6 to 14 per cent.), by no means easy work.

Having finally succeeded in establishing, out of a long series of *correct* breaking lengths, the values of B (best) and W (worst), we are puzzled by the fact that the best results are given by white silks, while we know from experience that yellow silks are of superior quality, as is also confirmed by the testing for cohesion. Also this contradiction can be explained, but it would lead too far to follow the long way necessary for this purpose.

The final result of our researches is: Excellent breaking length is only a proof that the thread was well stretched in reeling (fourth rule), which, however, is not sufficient for producing a really excellent thread; the essential condition for the latter is good cohesion, which can be arrived at only by observation of the ninth spinning rule.

If, therefore, a silk thread shows:

a. Good breaking length and bad cohesion, the first is to be considered as deceptive.

b. Breaking length and cohesion of the same degree; then the former is superfluous.

c. Bad breaking length and good cohesion; only in this case the breaking length would become interesting, for reasons to be explained in the next chapter.

But it seems that this case does not occur; at least as far as our experiences go. If further experiments should prove that it never appears in reality (and it is hardly conceivable by what class of cocoons or what method of spinning it should be brought about) we shall abandon the testing of tensile strength as we have abandoned that of ductility.

Chapter XI

THE RESULTANT

We have now tested all the various forms of the original defect of silk, the bale is completely analyzed, and having examined the seven indications of Degrees Serivalor, the buyer cannot be in doubt any more for which purpose the bale may or may not be employed.

Thus the technical side of the question is solved, but not its commercial side. For the seven figures of S° are often in contradiction to each other, and in order to express the commercial value of the bale, to say whether it is "extra or "first order," etc., it is necessary to reduce those seven figures to one, which we call the "resultant."

At the first glance this seems very easily done by taking the average of the seven components. But supposing an extreme case, f. i., that the components were: 1, 1, 1, 1, 1, 1, 10, they would give the average of 2.29, that is to say, a bale that is extremely bad in

one regard (f. i., flocks S° 10) would turn out as Resultant 2.3 nevertheless, viz., as "Extra." It is evident that this would not do.

Proceeding on this line, we soon find out that the loom is sensible only to the bad qualities, accepting the good ones as granted, just as a man whose one tooth aches does not heed the fact that his other teeth do not ache.

Now we are tempted to jump to the opposite extreme and to say: A bale which shows S° 10 only in one regard, must be designated by Resultant 10. But then we must ask ourselves: Is such a bale really of the worst quality possible, as would be expressed by Resultant 10? What would then remain for the following cases:

1, 1, 1, 1, 1, 10, 10, or
1, 1, 1, 1, 10, 10, 10, and so on, up to
10, 10, 10, 10, 10, 10, 10,

which last example only would represent the *worst possible* quality, from which the others must be distinguished by their Resultant?

Reflecting on the matter we arrive at the conclusion that the influence of each of the single degrees on the Resultant must be proportionately the greater, the worse this degree is. The mathematical way in such cases is to divide the sum of "powers" by the arithmetical sum of the degrees. There remains to find out which power should be employed; various experi-

ments have proved that only the second power can be right, and consequently we employ this one.

Considering that there can hardly exist a bale of silk that would get S° 1 on each of the seven points of testing, we see that Resultant 1.0 will never occur. In fact, degree 1.5 is practically the best and very rarely occurring Resultant. On the other hand, degree 10.0 is nearly impossible as well, and Resultant 9.5 is practically the worst that exists.

Between 1.5 and 9.5 lay eighty tenths of degrees, which with regard to their "constancy" (see next chapter) are reduced to forty, and by this we are arrived at the forty qualities of which we spoke in the Prospectus. How to calculate the commercial value of these forty qualities will be explained in another chapter. Here we follow with a table showing the relation of the usual designations of "Extra," "Classical," "first order," etc., to the degrees of the Resultant. Between "Extra" and "Classical" there is a considerable difference in price. In the following table resultant 2.5 is still "Extra," while 2.6 is "Classical." The real difference in value, however, between 2.6 and 2.5 is not greater than that between 2.5 and 2.4, or 2.6 and 2.7, etc.

The degress of Resultant indicate, therefore, as will be explained in one of the next chapters, the real commercial value which is not expressed by the names of the "chops" even if they are genuine, which is not always the case.

In order to deserve their designations, the "chops must not receive a worse degree of Resultant than indicated in this table:

Resultant up to So	2.5	3.0	3.5	4.0	4.5	5.0	5.5	6.0	6.5	7.0	7.5	8.0	8.5	9.0	9.5
Europe:	Extra	Classical			1	2		3							
China:	Extra		1			2			3						
Japan:		Double Extra		Extra	1	1	1-1½	1½	1½-2	2	2½				
Canton:						Extra		1			2			3	

Chapter XII

THE CONSTANCY OF THE DEGREES SERIVALOR

As each of the seven Serivalor testings is based on other facts and other methods, the "Constancy" of their results cannot be the same. The differences, however, are not great enough to make it necessary to establish a special table for each of them. It will be sufficient to state the average constancy as follows:

If a hundred testings of the same bale would give the average of S° 3.0 in one of the seven items, then:

about	20	of	these	testings	will	give	S°	3.0			
"	30	"	"	"	"	"	"	0.1	above	or	below
"	25	"	"	"	"	"	"	0.2	"	"	"
"	15	"	"	"	"	"	"	0.3	"	"	"
"	10	"	"	"	"	"	"	0.4	"	"	"

The Resultant is still more constant. Under the same supposition, of a hundred testings:

25 will give S^0 3.0
50 " " " 0.1 above or below
25 " " " 0.2 " " "

The average uncertainty of the resultant is therefore:

$$\frac{25 \times 0 — 50 \times 0.1 — 25 \times 0.2}{100} = S^0\ 0.1$$

which deviation occurs as often toward one side as toward the other, so that it appears neutralized.

It is better, nevertheless, to reckon with at least half of this uncertainty and to keep in mind, therefore, that Resultant 3.0 might be considered as being also 2.95 or 3.05. By this the gradation of eighty tenths of degrees, set forth in the last chapter, is reduced to forty, corresponding each to a difference in value of about five cents, as said in the Prospectus.

The question of value will be treated more explicitly in the following chapters.

Chapter XIII

THE DIFFERENCE BETWEEN THE MERCANTILE AND THE REAL VALUE

These two generally are considered as identical, but they are so only for the reeler and the dealer and not for the manufacturer.

To those who are producing or buying silk, the "real value" is given by the possibility of selling with profit, and as this possibility is dependent on the cost-price, the "real value" is founded on the latter. It is different with the manufacturer, who can sell only after having produced a new article of the raw material; for him the possibility of selling with profit is dependent on the fact whether the new article is well made or not. Here, then, the "real value" is connected with the way in which the material is adapted for its purpose and with the final result it produces; the "mercantile value," that is, the cost-price, is of secondary importance.

For example: In order to produce a good and comparatively cheap plush I must employ for the pile a glossy and well-covering material.

The best in this regard are "Bengal" and "Canton," and consequently they have the greatest "real value" for me, much greater in this case than "Italian Extra," f. i., although the "mercantile value" of the latter is much higher. Only after having resolved to buy one of the said sorts, I begin to take interest in their "mercantile value." For though in this case their "real value" exceeds that of Italian "Extra" I would not pay for them the same price, of course, knowing that I can buy them much cheaper. On the other hand, having to produce a Grège-Ottoman with forty-five splits, four threads to the centimeter, the "real value" of "Canton" is naught, as it is absolutely unfit for this purpose; but its "mercantile value" remains unaltered because of this.

The mercantile value of silver is subject to great changes, while that of gold remains rather constant; but their "real value" would be naught on a desert island, where some dates could preserve my life and therefore would represent the greatest "real value" for me. Hence follows:

1. The "mercantile value" of silk results from the comparison of its quality to its *price;* its "real value" from the comparison of its quality to its *employment.*

2. Those who are ignorant of this employment (reelers, throwsters, dealers) are unable to know the "real value."

3. A testing system that establishes Quality allows the fixing of the "mercantile value," and, indirectly, also of the "real value," which, however, differs according to how the material is employed.

Chapter XIV

THE MERCANTILE VALUE

The mercantile value of an article whose price changes daily can, of course, be indicated only relatively, that is to say, in the following way: A bale whose Resultant be, f. i., 4.5, is worth X per cent. more than the day's quotation for Resultant 5.5 (Japan 1 1/2). This X in reality is equal to 5 per cent., that is to say, each degree of Serivalor is equivalent to 5 per cent. With the aid of the following table the worth of each Resultant can be calculated, after having ascertained by how many per cent. the quotation of f. i. Japan 1 1/2 differs from the value indicated by the table for its Resultant 5.5: $3.77.

These prices represent at the same time the average quotations of the last fifteen years. We see that the value of S° 4.1 is doll. 4.04, that of S° 4.3 doll.

4.00; the intermediate two-tenths of a degree are to be considered only as one with regard to "Constancy" (see Chapter XII) the value of which is four cents, as stipulated in the Prospectus.

RELATIVE MERCANTILE VALUE OF THE RESULTANTS, IN DOLLARS.

(Tenths of Degrees.)

Deg.	.0	.1	.2	.3	.4	.5	.6	.7	.8	.9
1....						4.58	4.56	4.54	4.52	4.50
2....	4.47	4.45	4.42	4.40	4.38	4.36	4.34	4.32	4.30	4.28
3....	4.26	4.23	4.21	4.19	4.17	4.15	4.13	4.11	4.09	4.07
4....	4.05	4.04	4.02	4.00	3.98	3.96	3.94	3.92	3.90	3.89
5....	3.87	3.85	3.83	3.81	3.79	3.77	3.75	3.73	3.71	3.69
6....	3.67	3.65	3.64	3.62	3.60	3.59	3.57	3.55	3.54	3.52
7....	3.50	3.49	3.47	3.45	3.44	3.42	3.40	3.38	3.36	3.35
8....	3.33	3.32	3.30	3.29	3.27	3.25	3.24	3.22	3.21	3.19
9....	3.18	3.16	3.15	3.13	3.12	3.10				

The great difference in value between, f. i., Resultant 4.0 and 1.5 might call forth the question whether this difference is justified by the conditions of production. The answer to this question is not quite simple. It is, directly, no, indirectly, yes.

From the direct point of view, that is to say, considering the higher costs resulting from the better quality of cocoons and the greater care of spinning, the price of "Extra" appears exaggerated. The reeler, however, does not know his own cost-price. It is by no means easy to calculate, neither is the reeler

very much interested in it, seeing that his selling price is not dependent on his cost-price but on the quotations of the market. This makes nearly all reelers conceive the strangest ideas about their cost-price, and there are not two among 100 of them who would agree on this point. After long studies the author has come to the conclusion that from the changing wages, price of coal, and worth of the residual products, there might be calculated an average of lire 3.00 to the kilo (twenty-five cents to the pound) for wages and general costs, if the selling expenses are not too high, that is to say, if the reeler does not, f. i., keep a special office in Milan for selling a small production, or if he has not to pay too high interests to the Commissioner for the advancing of money for buying cocoons.

In order to find out the difference between the cost and the selling-price, it is necessary to compare the averages of the quotations of cocoons on the one hand, and of silk on the other, during a long period. This difference gives the selling price of the reeler's work, viz., what costs him lire 3.00. This selling price was, on the average of the last 10 to 15 years, lire 3.50, that is to say, the average gain of the reeler, who is exposed to a thousand risks and perils, was up to 1914-15, 50 centesimi to the kilo, that is four cents to the pound. If we consider that the Italian reelers, whose number is about a thousand, had paid the crop of 1914 about 175 millions of lire, and that, by the

collapse of prices caused by the great war, they have lost at least thirty millions, we must acknowledge that in general they are not in an enviable condition.

And now we are arrived at the "indirect" side of the question, whether the high price of "Extra" is justified.

The great majority of Italian reelers get on with but small capital, slight credit, at times high interest, and have no constant customers. They do spinning for stock, and as they cannot obtain good prices, as nobody would give them credit for producing "Extra," even if they did so, they are always working at great speed, trying to arrive at a high production under any condition, that is to say, they are producing qualities of Resultant 4.0 to 5.5, while many among them would be able to produce Resultant 2.0 to 3.5, if buyers would only allow them one to two lire more to the kilo (eight to fifteen cents per pound).

Nay, they are not only as able to spin as excellent silk as the most famous "Marca" reelers, but they are in better condition to do so, as they quite generally manage their establishments themselves, together with wife and children, while the "Marca" reeler generally lives in Milan, rarely visiting his establishments and completely relying on his staff for their management.

Thus we see, on the one hand, a majority of hard working people who are scantily recompensed for their labor, on the other, a small minority of wealthy men

who are independent enough of the market that they can refuse to sell if the prices of the day allow them too small a profit. They consequently demand not only the advance of one to two lire justified by their higher costs (some of them believe this difference to be twice as great), but they claim also the legitimate profit of their tiresome and risky work, of which the small reeler is robbed by his helpless position.

In this indirect way we come to the conclusion that the higher price of the better qualities is not unjustified.

In spite of this it is possible for the dealer or the manufacturer to buy good silks at a price only from one to two lire higher than that of "1st order" if, recognizing the reeler's position, he will treat him cleverly and benevolently, and also allow him the few weeks necessary for improving his products with the help of a good testing establishment, and if, after having arrived at this point, he keeps him constantly occupied with his orders, so that he may not be compelled to work on stock again.

It is necessary, moreover, not to change too often the sizes ordered, and not to demand a size which the reeler cannot produce, because the cocoons reared in his neighborhood do not yield it. The richer reeler is able to defend his interests in this regard and does not accept a size, for which his cocoons are not fit; the poor one is often compelled to give in. His cocoons have, f. i., a "bava" of 2.8, and he knows

that five of them yield a good 13/15. Now the buyer orders 13/14; instead of refusing the order he arranges a way of spinning and gives the order to combine three big and two small cocoons, that is to say, he deliberately acts against the second spinning rule, and the result is bad silk. Another expedient for him would be the well-known twofold sizing, but also this is less practicable for him, because he cannot run the risk of having the bales refused if the trick should once fail.

Finally the buyer must not induce the reeler to speculations, demanding of him to accept orders on long delivery in a moment when prices are low. The rich reeler is leading a continual war in this respect with his buyers, a war which in the course of years brings about as many victories as defeats for both parties, but which embitters them both. In the end both of them have hit it as often as not, and the outcome of the long silent fight is that neither has won anything. But in the rich reeler the buyer at least has a solvent opponent who can pay when he loses; but what can he claim from a poor man? If he has succeeded in enticing him to accept a disadvantageous contract, he only compels him to buy bad cocoons, in order to save himself from ruin, if possible, and consequently to produce bad silk. The latter will be recognized as such by a reliable testing establishment, but it has not become better for this. The buyer has the right to cover himself on the reeler's expenses,

but it is a hard thing to obtain payment of the difference from a man of scanty means.

The right way of concluding contracts is to base them on the official quotations, for instance: The reeler must deliver a quality not worse than Resultant 3.5; the price to be paid is calculated by the average quotations for "1st order" of the last three months preceding the day of delivery, with an advance of 1 to 2 lire.

Anybody who will compare the prices of his purchases of the last five years, f. i., with those he would have obtained by this method, will find that on the average he would have paid less. He would have saved himself the trouble of hitting the right moment and all the excitement connected with this system, and instead of the usual difference of three to five lire between "Extra" and "1st order," he would have paid only one to two lire.

There are very important manufacturers who have been buying according to this system for many years, and they evidently find it profitable, for they stick to it.

If the advantages of an absolutely reliable testing system are great with regard to the Mercantile-Value, they are still more so with regard to the Real Value, which will be the subject of the next chapter.

Chapter XV

THE REAL VALUE

The author is not informed in regard to the American wages for winding, and being driven into exile by the war, it is at present impossible for him to get exact information. He will therefore try an average calculation.

Suppose a winding-girl working with forty reels at a speed of 150 yards the minute, and arriving at 66 per cent. effective production, is paid sixty-five cents for ten hours of work; she will produce 1540 grammes of 13/15 in ten hours, and consequently the wages would be equal to twenty cents per pound.

In no case can this supposition differ far enough from the actual to seriously affect the following conclusions.

Generally there are: The wages per lb. for warping are 2½ times those for winding; the wages per lb. for weaving are 10 times those for winding, and the general expenses, including selling expenses, twice

the wages for weaving. So we have: Winding 20 cents, warping 50 cents, weaving $2, general expenses $4 per pound. All these wages and expenses ought to rise and fall in inverse proportion to the production, that is to say, if the material yields to a double production the wages ought to be reduced to a half.

With daily pay this reduction is brought about automatically, but with piece-work the adaptation is not quite so easy, and therefore we will leave this factor aside for the present. But doubtlessly the reduction is brought about in the general expenses, and these will be diminished therefore by twenty cents per pound if the production is increased by 10 per cent.

Now it is ascertained by experience that an increase of the production by 10 per cent. is caused by employing, for the same article, a quality of silk (raw or organzine) by one degree of Resultant better than the one employed before, as, f. i., Resultant 3.0 instead of 4.0.

Therefore the Real Value of one degree of the Resultant is, by diminishing of general expenses alone, about twenty cents.

Besides there ought to be also gained, in the course of time, three-quarters of the difference in wages for piece-work, while the last quarter ought to go in favor of the workmen. For by the *constant* employing of good silk also the wages for piece-work may be reduced without complaint of the workmen, if the thing is done benevolently, that is to say, in a way

that at the end of the year the weavers have earned more than before. This can be easily brought about by the said system especially on the occasion of fixing the wages for new articles. It is true that it can be correctly done only if the wages are calculated "a priori" quite justly and exactly, a task to which not all managers are equal. Perhaps that later on I shall publish a system of such calculations. In my own experience as a manager the results were:

After three years, and after having reduced the 135 hand-looms of the establishment to twenty-four, the general expenses were diminished by $10,000 (= 17 per cent.) while the average gain of the hands was increased from $95 to $110 yearly (= 16 per cent.) and the yearly production of each loom was raised from 3,000 yards to 4,500 yards (= 50 per cent.) with a greater proportion of good qualities. Each of the 20,000 pieces produced was diminished in cost by $1.50 for wages and general expenses, in comparison with those of three years before, the whole profit amounted therefore to $30,000.

It is true that this success may be ascribed to the circumstance that the whole organization was rather imperfect, so that there was occasion for many improvements. But the facts set forth here prove that two objects apparently opposed to each other may be obtained at the same time, *viz.*, diminishing of wages for piece-work, and increasing the workmen's gain.

It is evident that the workman does not consider how many cents he gets to the yard but only what his pay is at the end of the week.

In this regard I had two experiences: When I took up the management every technical improvement was considered as a hostile act; long years of suffering under narrow-minded management had taught the workmen that every change aimed at a diminution of their earnings, and consequently they had to be compelled into every reform or improvement. On the contrary, when I had been there for two years, I might have proposed to the men a diminution of wages by 10 per cent., and they would have accepted it readily, knowing very well that every change meant an increase in their earnings.

Regarding the relations between manager and workmen many things may have changed in the meantime in Europe, and it might have been different in America all along, but the fundamental conditions of the struggle for life are the same everywhere, and therefore from what I said before there results, that by employing the degree of Resultant which with increased speed yields the most efficient production, the better degree must turn out to be the cheaper, in spite of the fact that it costs more to the pound.

Thus the Real Value of a degree of the Resultant might be estimated as: 20 *cents, and* 3/8 *of twenty-seven cents* = 30 *cents; but for prudence sake we will say twenty-five cents.*

Of course the advantage can be obtained only if the speed of the looms is increased proportionally to the better quality of silk *and if this higher speed is maintained constantly;* which again is only possible if every bale has been thoroughly and exactly classified before.

It is not indispensable that this classification should be done by the Serivalor-system, if it is only exact and constant; but in this case it will needs follow the principles set forth in this publication.

There is another advantage of a just and impartial testing system, which I might call the prophylactic one. Just as everybody will prefer remaining well to being cured, it is better to receive a good quality than to be compensated afterwards for the loss on a bad one.

The reeler who knows that he is under control will in most cases produce good silk. Although he cannot be quite sure of the quality of his product, he, and especially his working people, who very soon begin to feel the control, are able to eliminate, by care and attention, nine-tenths of the causes of bad production.

But in order to arrive at this, it is necessary that the testing should be performed by an impartial establishment, that is to say, neither by the buyers nor by the reelers themselves.

The two parties would hardly agree, especially in times of great fluctuations of prices, and they would

have to refer the matter to an impartial institute, which must be maintained by the whole trade, and which would be obliged to demand high fees if applied to only in cases of dissension. But the moment the public institution exists, every private testing becomes superfluous, and therefore all endeavors to invent testing methods which could be employed by anybody in his own house, without studies and without instruments, are useless, besides that they must needs remain unsuccessful, as will be recognized by everybody who has followed me so far.

It may be that many of my readers will be under the impression that the clerks of a testing establishment according to the Serivalor system must be learned men or at least a M. A. of an university; nothing of the kind is required, just as it is not necessary to be a mathematician for using logarithmic tables. The collecting of the material and the calculating of the Serivalor tables required many years of work, and their use demands only intelligence and attention.

If private testing were of any use, why is the establishing of the weight left to the Conditioning Houses? Certainly not because the apparatus is too costly or its handling too difficult. A manufacturer who buys 100,000 kilos of silk yearly pays, in Europe, 3,000 francs for their conditioning, while the apparatus costs only 500 francs and the work may be done by any of the clerks. Nevertheless nobody thinks of hav-

ing the conditioning done in his own house. How much less logical is this for the far more difficult and complicated testing of quality!

Also the conditioning houses have not yet been in existence a long time. I tried in vain to find out the day of their origin in Milan, but according to all information they cannot be even a hundred years old.

How was it before? Certainly many people who produced silk at that time cheated as much as they could by moistening. But no doubt there were also honest men who would have preferred fair trade; these and the dealers who were the injured ones in this case, must have conceived the idea of an official conditioning institute—an idea the execution of which may have met with no small difficulties.

It were certainly not the honest men who opposed it—just as to-day there are no honest men who are opposed to the idea of an official testing institute—but finally the thing was carried through, and to-day we cannot imagine the silk trade without the conditioning houses.

Doubtlessly it were chiefly the dealers, whose interests were at stake, who brought about the innovation. But no immediate interest either of the producer or of the dealer demands a change in the testing of size and of quality, although they both would certainly profit from a solid and honest basis for the trade, after the "Testing House" had been in action for a certain

time. But meanwhile they are not so injured by the present state of things that they should be in a hurry to alter it. They leave the matter to itself and confine themselves to proposing little reforms of usages, etc., to conferences and congresses. In all these assemblies only producers, throwsters and dealers are to be seen or heard, but hardly a manufacturer. Among the more than 100 members of the International Congress of Torino, 1911, there was only one manufacturer; America had not even sent a single representative; why this?

Because the manufacturer is too much occupied with the cares of production and selling to remember that *the surest profit is that made in buying;* and he has, as it were, *no time to defend his interests.*

And yet the reform of the silk trade with regard to the testing of Quality and Quantity must be initiated by the manufacturer, as it is *his* interest that is at stake in this regard. In the centers of production: Yokohama, Shanghai, Canton, and Milan, there exist only reelers and dealers, but no manufacturers, and it is quite natural that the usages established in these places are in accordance with the interests of those who made them.

But there is a town where the manufacturers form the enormous majority and whose consumption is so preponderant that it commands the attention of the whole trade.

What is earnestly claimed by New York will be

accepted by all markets as readily as there ever was accepted a just and rational reform, and therefore to the author it appears as a duty of the American manufacturers to take the lead in the question of the *Reform of the testing of Quality and Quantity.*

They may be sure that by doing so they will earn the gratitude of their European brethren, and finally also of the producers and dealers.

We have now exhausted our main subject, the right valuation of silks. The following chapters will treat technical questions in connection with silk, but not with its value.

This reproduction from the original photograph shows the difference between split and unsplit ends.

Chapter XVI

LOUSINESS

Dyed silk not rarely shows a light-hued down to which was given the appropriate name of "lousiness." The causes of this defect remained unknown for a long time. We shall try to give an account of the respective researches.

The cocoon-thread is composed of two "elementary threads," which on their part consist of countless little fibers. For a long time this structure of the elementary threads was contested, and even L. Blanc in his excellent study (1) has still asserted, with all his authority, its homogeneity. But Italian and German scientific men continued to furnish new proofs to the contrary, and finally A. Conte and D. Levrat succeeded in proving the fibrillous structure (2) by especially

(1) *Etude sur la sécretion de la soie. Annales du Lab. d'études de la soie. Lyon* 1887-88.

(2) *Sur la structure fibrillaire de la soie. Annales* 1901-02.

conclusive experiments, so that this question appears theoretically solved.

Weavers may persuade themselves in the following way: Decomposing the warp-threads of a piece-dyed stuff under the microscope, we discover, among the elementary threads of various size, some which distinguish themselves by their very small diameter. Measured micrometrically, their diameters appear as 1/5 to 1/10 of those of the rest, they consequently possess only 1/25 to 1/100 of the body of ordinary elementary threads, and this fact alone must call forth reflections about their origin.

Moreover, in counting the elementary threads, we find that very often they appear odd-numbered, and as the cocoon-thread always consists of two elementary threads, we see that one or several of these must have been split. Following the thread with the microscope-needle, we soon arrive at the point where the split-off fiber unites itself with its original thread, and by this we have the proof of its fibrous structure.

The photo on page 142, taken, by kind permission, from a publication of the Laboratory of the Stagionatura Anonima Milan, shows the difference between split and unsplit threads.

The splitting, however, occurs in raw silk, and all defects of the latter consist at least of one intact cocoon-thread. It is therefore useless to examine raw silk with regard to the question whether it will become "lousy" in the dyeing, *for this defect is caused solely*

by split elementary threads. It has nothing to do with the quality of the silk, *but is a fault of the dyer's.*

Although this defect is as old as dyeing itself, it was not before 1896 that it began to be scientifically examined. From the studies published on this subject by the Laboratory of the Milan Stagionatura Anonima under the direction of Professor Gianoli, and by Professor Lenticchia of Como, (1) we gather:

(1) *Lenticchia: "Sopra un nuovo difetto della seta di Bombyx mori."* (*Boll. di sericoltura* 18 *and* 25, *May,* 1896.)

Gianoli: "Intorno alla imperfezione degli attuali sistemi di tintura della seta." (*Bol. d. ser.* 6, *March,* 1898.)

Gianoli: "Communicazione preliminare sulle cause che provocano lo sfilacciarsi delle sete tinte." (*Relazione presentata al* 4°. *Congresso di Bacologia e Sericoltura in Torino* 1898.)

Laboratorio della Stag. an. Milano: "Intorno al difetto di sfilacciarsi di un filato di seta tinta." (*Boll. d. ser.* 27 *Mai,* 1900.)

Laboratorio: "Intorno allo sfilacciarsi delle sete durante le operazioni tintorie." (*Boll. d. ser.* 10, *March,* 1901.)

Lenticchia: "Nuove osservazioni ed esperienze sulla formazione dei fiocchetti nella seta del filugello." (*Como* 1902.)

Laboratorio: "Appunti alle osservazioni del Prof. Lenticchia." (*Boll. d. ser.* 13, *April,* 1902.)

Lenticchia: "Ancora sulla formazione dei fiocchetti della seta." (*Como,* 1902.)

Laboratorio: "Ancora sui fiocchetti delle sete." (*Bol. d. ser.* 30 *Nov.*, 1902.)

Lenticchia: "Sempre sui fiocchetti della seta." (*Como,* 1902.)

Lenticchia: "Sulla forma, composizione e struttura del filo serico." (*Milano,* 1903.)

Chapter XVII

LUSTER AND COVER

Also these two qualities can be judged according to the Serivalor system, and our laboratory will do the testing on demand; but, being without connection, nay, sometimes even in opposition to the other Degrees Serivalor, they would make appear a wrong Resultant, and therefore cannot be comprised in the latter. For this reason it would also be useless to explain the way of testing them; it will suffice to give the following general indications.

The luster of the thread and its capacity to cover the tissue are qualities of race and therefore independent of the method of reeling.

They are proportional to each other, that is to say, the more lustrous a thread is, the better cover it will yield, and vice versa. But the luster of raw silk has nothing to do with that shown in the tissue.

For both qualities the twisting is the decisive factor. Therefore, raw silk can be compared only to raw silk and thrown silks only to others of the same

twist, but the comparison must never be done in the skein.

Dyed simultaneously and treated the same way, all skeins of raw silk will show the same luster, and so also all skeins of thrown silk of the same twist.

But even in the tissue the difference between more or less lustrous silk is diminished:

(a) The heavier the count,

(b) The sharper the torsion is.

More lustrous materials yield a cover of high luster in single weaving, and also a more lustrous tram, but with an organzine of 550 turns to the meter, the difference is hardly perceptible any more. The higher luster of the tram will be visible only in light weft tissues, and also with single-warp satins the difference is more striking in forty splits two threads, than in forty splits six threads to the centimeter.

Under condition of the highest luster (as with light-count satins), the relation of the extremes is: Fifty threads of S°1 have the same lustrous effect as seventy-two threads of S°10, of the same size.

But this does not mean that two tissues woven of materials of this proportion would have the same aspect. For this it would be necessary that the cover should be also equally *deep*. Silk is diaphanous, and the experienced eye easily distinguishes the thin, shallow size from the thicker, deeper one.

Therefore, the proportion established above is valid only for effects of the surface as dent-streaks, etc.

Chapter XVIII

THE TOUCH

In general the touch of the tissue is in inverse proportion to its luster, and proportional to the S° of cohesion of the thread.

It depends, however, more on the race than on good reeling.

China silk yields a perfect, that is to say, rich and at the same time soft, touch; next to it come:

Japans; then Toscana and Turkestan.

Satisfactory in regard to richness of the touch, but not to its softness, are: Levante, Gialli puri, Caucasians, Incroci Chinese.

Of firm, but rather hard and rough touch, are: Incroci Giapponesi, Persia.

Bengal and Canton yield a flabby but smooth touch.

In regard to this quality no gradation of testing is established.

Chapter XIX

THE LOSSES BY PREPARATORY PROCEDURES

(A) *Winding*:

The loss by the winding is proportional to: The carefulness of the winding-girl; the circumference of the reel; the degree of Serivalor for winding.

With a careful girl and a circumference of 1½ meters, the loss is:

For S° Winding:

	1	2	3	4	5	6	7	8	9	10
Per cent.	0.2	0.4	0.6	0.8	1.0	1.2	1.4	1.6	1.8	2.0

(B) *Cleansing during the warping, or throwing.*

The losses are not important:

For S° Flocks:

	1	2	3	4	5	6	7	8	9	10
Per cent.	0.1	0.2	0.3	0.4	0.5	0.6	0.7	0.8	0.9	1.0

Lenticchia is of the opinion that the defect lies in the silk thread; Gianoli that it is caused by the dyeing. The latter states that flocks occur in every dyed skein, but that they remain unnoticed if their number does not surpass 150 in 1,000 meters of thread. By cautious treatment on bobbins he succeeds in dyeing nearly without flocks a small quantity of silk, of which the rest has come back "lousy" from the dyer.

Lenticchia objects that Gianoli's delicate method cannot be employed by the industry and asks: "Why, with the same treatment, one lot of silk becomes 'lousy' and the other not?" He believes the reason to lie in the weak constitution of the thread which may originate from worms that have suffered by disinfecting vapors. As this method is chiefly used in Italy, his supposition would diminish the value of Italian silks.

Gianoli, on the contrary, proves that Italian silks are not more subject to this defect than Asiatic ones. Lenticchia acknowledges this fact, but for the rest sticks to his opinion, which he supports by new results of his studies, viz:

(1) In the silk thread liable to "lousiness" the fibrin is not only surrounded, but penetrated by the sericin.

(2) Worms treated with disinfecting vapors produce a thread penetrated by the sericin and consequently gets "lousy" in the dyeing.

(3) The end of the thread towards the chrysalis is flatter and more penetrated by sericin than the rest.

Of this I wish to remark:

(a) It is true that microscopic flocks appear in nearly all dyed silks, but in order to form a technical defect they must be visible to the naked eye. Stretching a dyed skein so that it forms an even, glossy surface, and regarding it with his back to the light, one discovers the flocks as little pale dots.

Their lighter hue is owing to the small diameter of the split-off fibers which consequently are more diaphanous than the unsplit elementary threads. By the same optic law the foam of a colored liquid appears much lighter than the liquid itself.

My explanation was accepted later on by Professor Gianoli in a lecture given in the Chemical Society of Milan.

(b) To Professor Lenticchia's question, why, with the same treatment, some silks become "lousy" and others do not, I reply:

Just on account of the same treatment. If I should treat a race-horse the same as I might a pack-horse it would perish; but for this it is not to be considered as a degenerated animal.

(c) I have had dyed dozens of bales of Italian trams without finding flocks; but I found them very often in white "Levante," and also most of the law-suits concerning "lousiness" were caused by white silks. According to my experience I must therefore consider these silks: (Persia, Turkestan, Brusa), as especially liable to "lousiness," although there does not exist a sort which would be quite exempt from this defect.

(d) That fibrin penetrated by sericin is liable to splitting appears probable, but I don't believe that this penetration is owing only to the causes to which Prof. Lenticchia ascribes it, for the Persians do not use disinfecting vapors, and if the spinning of the cocoons up to the last bit were responsible for the defect, the latter would occur oftener with Italian silks than it does.

My assertion that "lousiness" is independent of the quality of raw silk was confirmed by the following experience: I tried to employ Brusa 12/14 for the warp of a satin of forty-five splits, three threads, to the centimeter, in order to see whether it would turn out very streaky. The winding, warping and weaving (width 130 centimeters, 130 strokes to the minute), went on regularly, and the piece came back faultless from the dyer's. The rest of the bale was employed for tram, and *this was dyed "lousy" by two important dyers.*

Not to mention that the testing of the said Brusa had given a good degree of cohesion, its good quality

was made evident by the fact that it could be employed for forty-five splits, three threads to the centimeter, a count of reed, for which good Canton is unfit, while trams even of the worst Canton may be dyed free of "lousiness."

Seeing, moreover, that the same material did very well for piece-dyeing, we come to the conclusion that—

(1) Raw silk of good quality may be liable to "lousiness" nevertheless.

(2) "Lousiness" is the consequence of a fault of the dyer's.

I tried myself to dye the said tram in the laboratory and succeeded in dyeing it with or without "lousiness" at will.

As Prof. Lenticchia had said in his article: "I possess a skein of silk; a wreath of laurel to him who is able to dye it free from 'lousiness' in the ordinary way (that is to say, not on bobbins)," I wrote to him to send me the skein, marking it with a sealed string. This string would also prevent the dyeing on bobbins. (In fact, I am employing sticks.) Prof. Lenticchia replied that he did not possess the skein any more, and congratulated me on my invention.

I presume, though, that the dyers know very well that the fault is theirs. This supposition seems to be confirmed by the fact that I received only one answer to the Prospectus of the first edition of my book, in which I had announced elucidation on this matter, and

which was sent to all the dyeing establishments in Europe. As I cannot believe that these establishments do not take interest in new publications concerning their industry, there is no other supposition left but that they did not want to spend money to learn what they knew already.

To sum up:

There are silks which, independent of their quality, are more liable to "lousiness" than others; but even those can be dyed free of this defect.

Chapter XX

THE DIAMETER OF THE SILK THREAD

The diameter of the same size is different according to the number of cocoons employed for it. This number varies considerably according to the race: Size 13/15, f. i., may be made of 4, 5, 6, 7, 8 or even 9 cocoons (Canton). The differences occurring may be calculated in the following way:

We suppose a silk thread to be composed of four cocoon-threads (bava) whose diameter be 1.0 and whose weight equally 1.0. If the same size should be made of 5 "bave," their weight must be 0.8 ($4 \times 1.0 = 5 \times 0.8$) and the diameter will result from the equation:

$$1.0 : X = \sqrt{1.0} : \sqrt{0.8}, \text{ which gives}$$
$$X = 0.9$$

In the first case we unite 4 "bave" of diameter 1.0, which give a smallest diameter of 2.0, while in the second case 5 "bave" of the diameter 0.9 are

united, which must give a diameter of more than 2.0, as can easily be seen by anybody who will group together 4 and 5 disks of these proportions. In fact, the smallest diameter of 5 threads must be 2.34.

Putting 100 for the smallest diameter possible, we have for:

Cocoons:	4	5	6	7	8	9
Diameter:	100	117	103	113	115	100

Another factor of the diameter is the more or less complete stretching of the bava, of which the quality of the thread is dependent, and therefore it might be said in general: The smaller the diameter of the thread is, the better its quality; but we have seen in the respective chapters how difficult it is to judge by these indications.

Apart from these restrictions, there are two ways of finding the average diameter of a silk thread, both by supposing the thread to be a cylindrical body and consequently having a circular transverse section.

First method:

The diameter of homogeneous cylindrical bodies can be derived from their specific weight. Accepting the silk thread as an homogeneous body, by means of the pykometer, I found its specific weight to be 1.29 to 1.30.

The diameter (D) of cylindrical bodies being proportional to the square root of their weight,

$$D = X \sqrt{\frac{10S}{9}}$$

and from the specific weight of 1.3 it results that X = 10 (expressed in microns = thousands of millimeters).

(L. Vignon in his "*Recherches sur la densité de la soie*" (*Annales du Laboratoire d'études de la soie, Lyon,* 1889-1890) had established a specific weight of 0.9 to 1.1 by the mercury method. A year later, by immersion into benzine, he found 1.33 to 1.34. Vignon rejects the method of the pykometer, as he thinks the sericin must be dissolved by the boiling water. I beg to object to this, that during the few moments that the experiment lasts the solution must be so slight that it influences the correct result much less than the friction of the skein against the benzine, which diminishes the sensibility of the balance. The mere fact that the result obtained by my method is smaller than his will furnish to every physicist a sufficient proof that mine must be right.)

As, however, the silk thread is not homogeneous, but a bundle of roundish bodies, X must be greater than 10.

(The transverse section of a cocoon-thread might be described as two isosceles right-angled triangles with rounded-off corners joined together. Microscopic

photos are to be seen in: *"Notice sur le laboratoire d'études de la soie. Condition des sois, Lyon.* Others on a larger scale in Prof. Lenticchia's *"Sulla forma, composizione e struttura del filo serico," Milano,* 1903).

Supposing the cocoon-thread to be of cylindrical form, X must be at least as much larger than 10, as the outward square is larger than the inward circle:

$$X : 10 = (2r)^2 : r^2 \pi$$

Consequently, X must be at least 12.7. But, as said above, the cocoon-threads are not of completely cylindrical but of roundish shape, and therefore cannot be joined together as tightly as it would be possible with cylinders.

Under the microscope the gaps between them are clearly visible. (See Chapter VIII, Cohesion.) From this results that X must be larger than 13, and it may be concluded that it cannot be much below 14.

Second method:

The diameter of cylindrical bodies might be measured directly by joining them together without gaps on a fixed length; in our case, by winding the thread around a dark board. If a thread of a certain length, in meters, (L) and a certain weight, in tenths of milligrams (W) joined together F times, covers a certain length in microns (S) on the board, then:

$$X = \frac{\frac{S}{F}}{\sqrt{\frac{W}{L}}}$$

The winding of the thread around the board requires much patience and a certain skill, both of which qualities are not rare with weavers.

After some effort we succeed in joining together the threads so that under eightfold linear enlargement they showed neither gaps nor doublings. The flattening of the threads is avoided by their comparative hardness. By this experiment we arrive at the following figures:

"Piemont"	W = 35	L = 1.125	S = 1560	F = 20	X = 14.0
"China"........	W = 77	L = 1.125	S = 2860	F = 25	X = 13.8
"Brussa"	W = 22	L = 1.125	S = 900	F = 15	X = 13.6
"Bengal"	W = 48	L = 1.125	S = 1820	F = 20	X = 14.0
"Bengal"	W = 40	L = 1.125	S = 1720	F = 20	X = 14.4

These being sufficiently in accordance with the results of the first method, we may state

that $X = 14$.

(In the *"Annales du laboratoire d'études de la soie,"* Lyon, 1886 and 1887-88, are published many microscopic measurings of the diameter of the cocoon-thread. According to these, the value of X oscillates between 14 and 20. But, as it is not said whether these figures concern the larger or the smaller diameter, and as moreover the proportion of these two changes the more the thread approaches its end, those measurings do not affect the results we arrived at.)

Thus we may establish the following table of diameters:

Size		4	5	6	7	8	9	10
Mikron		29.5	33	36.1	39	41.7	44.3	46.7
Ten thousandths of inch = Mikro-inches		11.6	13	14.2	15.3	16.5	17.4	18.3
Size	11	12	13	14	15	16	17	18
Mikron	48.9	51.1	53.2	55.2	57.1	59	61	62.6
Mikro-inches	19.2	20.1	20.9	21.7	22.5	23.2	23.9	24.6
Size	19	20	21	22	23	24	25	26
Mikron	64.3	66	67.6	69.2	70.7	72.3	73.8	75.2
Mikro-inches	25.3	25.9	26.6	27.2	27.8	28.4	29.0	29.6
Size	27	28	29	30	31	32	33	34
Mikron	76.7	78.1	79.5	80.8	82.1	83.5	84.8	86.0
Mikro-inches	30.1	30.7	31.2	31.7	32.3	32.8	33.3	33.8

These figures are important for the weaver, as they indicate the space occupied by the warp-threads in the reed. Another table in the chapter concerning reeds will show the space left by the various reeds, and by a comparison between those two we recognize the increasing friction of heavy warps and the necessity of the better cohesion, the more threads are pressed together in the diminishing space.

Chapter XXI

THE ALTERATION OF SIZE BY THROWING.

There are two factors which alter the size during the throwing:

(A) The breaking of the thread during the winding and the following operations:

This breaking generally occurs at the weakest, that is to say, thinnest parts of the thread and is followed by the removal of a certain length; the thinner parts thus being eliminated, the rest has become heavier in proportion to its length. A rough estimate tells us that the influence of this factor cannot be great; it might be calculated in the following way:

The losses by winding and throwing vary, as put forth in Chapter XIX between 0.2 and 2%. We will suppose in our case, a loss of 1%, the size being 13. In a thread of middling Regularity the thinnest parts will be of about size 7; these parts will chiefly break, and their weight being 1%, it results:

At the beginning a gram contained 692 meters; of these were eliminated 0.01 gram = 13 meters (1 gram of size 7 containing about 1,300 meters) and there remained 0.99 grams = (692 — 13 =) 679 meters, which is equal to 686 meters to the gram. The alteration is then, in our case:

$$\frac{6}{686} = 0.9\%$$

of which we might derive, as a general rule:

The thickening of the size, by elimination of the thinnest parts of the threads, is equal to the loss, in per cent.

(B) When twisted the thread forms a screw-line which of course is no longer than the straight line. By following this longer way the thread becomes shorter and consequently of heavier size than it was before.

In order to calculate this thickening of the size we must start from the diameter of the thread, for which purpose, however, the figures found in the last chapter must be somewhat altered.

It is evident that the natural diameter of a soft body must become smaller under the pressure of the twisting. This diminution can be ascertained by a comparison between the diameters of the single and the twisted threads. If there had been no pressure, the latter would be (D = single diameter)

$$D\sqrt{2} = 1.414D$$

But it can be ascertained by the second method, explained in the last chapter, that it is only

$$1.21\ D$$

Consequently the diminution by pressure is 1/7, and in regard to the condition of the twisted thread X is equal to 12.

(This condition resembles that within the mercury by which Professor Vignon found the specific weight of 0.9 to 1.1, viz., compressed but containing air. It was presumable, therefore, that a calculation of the specific weight on the basis of X=12 would give a similar result. In fact, it is 0.9.)

My calculations of the shortening by twisting are therefore based on:

$$X = 12.$$

When two threads are twisted together each spot of their substance describes a curve which might be considered as the combination of two screw-lines. On the one hand each thread is turning around the com-

mon axis—and this causes its shortening—on the other hand it is turning around its own axis, the consequence of which is a stowing, of which we shall speak later on.

D being the diameter of the thread, P the progression along the axis during one turn, L the length of the screw-line, then

$$L = \sqrt{P^2 + (D\pi)^2}$$

the shortening (S):

$$S = L - P$$

and the thickening of size (T) in per cent.:

$$T = \frac{L - P}{P}$$

Consequently we have for the twisting of a thread size 13:

(a) Tram, 120 turns to the meter:

$$P = 8333 \text{ microns}$$
$$D = 45.6 \text{ microns}$$

$$L = \sqrt{8333^2 + (45.6 \times 3.14)^2} = 8334$$
$$S = 1$$
$$T = 0.012\%$$

(b) Organzine 425 turns to the meter:

$$P = 2353 \quad S = 4 \quad T = 0.2\%$$

(c) Grenadine 1300 turns to the meter:

$$P = 770 \quad S = 13 \quad T = 2\%$$

(d) Crêpe, 3000 turns to the meter:

$$P = 333 \quad S = 30 \quad T = 9\%$$

Adding to this the thickening by elimination of the thinnest parts, the increase in size, in comparison to the original thread, is:

For	Tram	¾%
"	Organzine	1½%
"	Grenadine	3 %
"	Crêpe	10 %

These figures are altered according to the losses in the winding.

With threefold throwing the increase must be the same if the number of turns is rationally fixed. This number ought to be in inverse proportion to the diameter of the twisted thread, or rather to the square root of its weight—the latter being proportional to the diameter.

Thus the number of turns (N) for threefold organzine of size 13 results from the equation:

$$N : \sqrt{26} = 425 : \sqrt{39}$$

which gives N = 347.

With this right method the greater shortening—

owing to the longer way of the three threads—is balanced by the smaller number of turns.

In order to avoid the stowing of which we spoke before, the single thread receives a preliminary turning in opposite direction of that of the twisting. But this well conceived expedient is turned into a disadvantage if, as it is often the case now, the preliminary turns surpass those of the twisting. The stowing then occurs the opposite way, the organzine gets a granular aspect, becomes less lustrous and covers less well.

Consequently the number of preliminary turns ought to be equal to that of the twisting.

We shall now convert the above theoretical knowledge into such form as will enable anybody to make the accounts connected with throwing.

We call:

Theoretical size (Th. S.) the product of the multiplication of the raw-size, with the number of threads to become united. Hence if we have to throw three threads of 11/13 (= 12) the Th. S. is always 36, independent of the number of turns we give them.

Of this Th. S. we must always draw the square-root ($\sqrt{\text{Th. S.}}$). To this purpose table A has been made, which contains the $\sqrt{\ }$ of all numbers from one to 200.

If we divide the number of turns to the meter (T) by the $\sqrt{\text{Th. S.}}$ the quotient we get forms the

very distinctive feature of the throwing. We call this quotient the coefficient (Co.). Table B shows the shortenings of the thread corresponding to the difference coefficients.

Table C shows the coefficients for the usual kinds of throwings.

We arrive from the number of turns to the meter to that one to the inch, dividing T with 40 $\left(=\frac{T}{40}\right)$ consequently, multiplying the number of the turns to the inch with forty, we shall get T.

(B) *Coefficients of throwing (Co.) and shortenings in % of the Theoretical Size (Th. S.).*

Co.:...	138	190	237	271	308	341	368	393	417	440	462	483	503
%:.....	½	1	1½	2	2½	3	3½	4	4½	5	5½	6	6½
Co.:........		522	540	558	576	594	612	630	647	664	680	696	
%:........		7	7½	8	8½	9	9½	10	10½	11	11½	12	

(C) *The usual throwings are the effects of the following Coefficients:*

Open, filling tram giving a soft touch..............Co. = 20
Hard tram, not filling giving a strong touch........Co. = 30
Open, lustrous organzine for satins.................Co. =100
Medium organzine, less lustrous for armures......Co. =120
Hard, lusterless organzine for taffetas..............Co. =140
Lustrous grenadine ....Co. =200
Lusterless grenadine.................................Co. =250
Lusterous crêpe ....Co. =400
Medium crêpe ....Co. =500
Hard crêpe ....Co. =600

TABLE A—THE SQUARE-ROOTS OF THE NUMBERS 1 TO 200.

n	$\sqrt{n}$	n	$\sqrt{n}$	n	$\sqrt{n}$	n	$\sqrt{n}$	n	$\sqrt{n}$	n	$\sqrt{n}$	n	$\sqrt{n}$	n	$\sqrt{n}$
1	1.	26	5.10	51	7.14	76	8.72	101	10.05	126	11.23	151	12.29	176	13.27
2	1.41	27	5.20	52	7.21	77	8.78	102	10.10	127	11.27	152	12.33	177	13.31
3	1.73	28	5.29	53	7.28	78	8.83	103	10.15	128	11.31	153	12.37	178	13.34
4	2.	29	5.39	54	7.35	79	8.89	104	10.20	129	11.36	154	12.41	179	13.38
5	2.24	30	5.48	55	7.42	80	8.94	105	10.25	130	11.40	155	12.45	180	13.42
6	2.45	31	5.57	56	7.48	81	9.	106	10.30	131	11.45	156	12.49	181	13.45
7	2.65	32	5.66	57	7.55	82	9.05	107	10.34	132	11.49	157	12.53	182	13.49
8	2.83	33	5.75	58	7.62	83	9.11	108	10.39	133	11.53	158	12.57	183	13.53
9	3.	34	5.84	59	7.68	84	9.16	109	10.44	134	11.58	159	12.61	184	13.56
10	3.16	35	5.92	60	7.75	85	9.22	110	10.49	135	11.62	160	12.65	185	13.60
11	3.32	36	6.	61	7.81	86	9.27	111	10.54	136	11.66	161	12.69	186	13.64
12	3.46	37	6.08	62	7.87	87	9.33	112	10.58	137	11.70	162	12.73	187	13.67
13	3.61	38	6.16	63	7.94	88	9.38	113	10.63	138	11.75	163	12.77	188	13.71
14	3.74	39	6.24	64	8.	89	9.43	114	10.68	139	11.79	164	12.81	189	13.75
15	3.87	40	6.32	65	8.06	90	9.49	115	10.72	140	11.83	165	12.85	190	13.78
16	4.	41	6.40	66	8.12	91	9.54	116	10.77	141	11.87	166	12.88	191	13.82
17	4.12	42	6.48	67	8.19	92	9.59	117	10.82	142	11.92	167	12.92	192	13.86
18	4.24	43	6.56	68	8.25	93	9.64	118	10.86	143	11.96	168	12.96	193	13.89
19	4.36	44	6.63	69	8.31	94	9.69	119	10.91	144	12.	169	13.	194	13.93
20	4.47	45	6.70	70	8.37	95	9.75	120	10.96	145	12.04	170	13.04	195	13.96
21	4.58	46	6.78	71	8.43	96	9.80	121	11.	146	12.08	171	13.08	196	14.
22	4.69	47	6.86	72	8.49	97	9.85	122	11.05	147	12.12	172	13.12	197	14.04
23	4.80	48	6.93	73	8.54	98	9.90	123	11.09	148	12.17	173	13.15	198	14.07
24	4.90	49	7.	74	8.60	99	9.95	124	11.14	149	12.21	174	13.19	199	14.11
25	5.	50	7.07	75	8.66	100	10.	125	11.18	150	12.25	175	13.23	200	14.15

First example:

Question: What will be the size and the character of the thread resulting from throwing 14 ends of size 10:55 with 170 turns to the meter (= 4¼ to the inch)?

Answer: (1) Th. S. = (10.55 × 14 =) 147.7 den.

(2) $\sqrt{147.7} = 12.15$ (see table A).

(3) $\text{Co.} = \dfrac{170}{12.15} = 140$

(4) Shortening (table B) of Co. 140 = ½%

(5) Size = Th. S. plus ½% = 147.7 + 0.7 = 148.4

(6) Character: Hard organzine (table C).

Second example:

Question: How many turns to the meter and to the inch must I give to the size 9/11 = 10, three thread, to become a lusterless grenadine, and what will be the final size?

Answer: (1) Th. S. = 4 × 10 = 30.

(2) $\sqrt{30} = 5.48$.

(3) Co. for lusterless grenadine = 250.

(4) T = 5.48 × 250 = 1370 turns to the meter = 34 to the inch.

(5) Shortening = 1¾%.

(6) Final size = 30.5.

Third example:

Question: With what raw size must I begin, to arrive at "Poile" size 16, 2400 T (60 to the inch)?

Answer: (1) Th. S. = 16.

(2) $\sqrt{16} = 4$.

(3) Co. = $\frac{2400}{4}$ = 600

(4) Shortening for Co. 600 = 9%.

(5) Raw size 16 minus 9% = 14.56.

Counter-proof:

(1) Th. S. = 14.56.

(2) $\sqrt{14.56} = 3.8$.

(3) Co. = $\frac{2400}{3.8}$ = 632

(4) Shortening of Co. 632 = 10%.

(5) Final size = 14.56 + 10% = 16.

(6) Character: Hard Crêpe.

Chapter XXII

THE REEDS

It might be presumed that there is no mill in which the reeds are in perfect order, that is to say, where all the dents are in right proportion to their count. I am led to this opinion by the fact that while I was a manager I had great difficulty in procuring such reeds, seeing how little the reed-makers were used to keep strictly to instructions. And yet the right size of the dents is so important that without accuracy in this regard no reliable results may be reckoned upon.

The friction of the silk against the dents increases with the number of threads, as the diminution of space produces a pressure, which, as proved by Coulombe, is proportional to the friction.

Therefore it is not indifferent how much space is taken by the dents, and the proportion of their size to the clear space between them must be taken into consideration.

TABLE OF PROPORTION BETWEEN DENTS IN THE CENTIMETER AND IN THE PARIS INCH.

Dents in the Paris inch:	50	55	60	65	70	75	80	85
Dents in the centimeter:	18	20	22	24	26	28	30	31½

Dents in the Paris inch:	90	95	100	105	110	115	120
Dents in the centimeter:	33	35	37	39	41	42½	44

Dents in the Paris inch:	125	130	135	140	145	150	155
Dents in the centimeter:	46	48	50	52	53½	55	57

Dents in the Paris inch:	160	165	170	175
Dents in the centimeter:	59	61	63	65

The "No." expresses the number of dents in the total size of the ¼ Paris inch (= 67½ Ctm.). Consequently the size of one dent is:

No.:	35	36	37	38	39	40	41	42	43	44
Microns:	193	187	182	178	173	169	165	161	157	153
No.:	45	46	47	48	49	50	51	52	53	54
Microns:	150	147	144	141	138	135	132	129	127	125
No.:	55	56	57	58	59	60	61	62	63	64
Microns:	122	121	119	117	115	113	111	109	107	105
No.:	65	66	67	68	69	70	71	72	73	74
Microns:	104	102	101	99	98	97	95	94	92	91
No.:	75	76	77	78	79	80	81	82	83	84
Microns:	90	89	88	87	86	84	83	82	81	80
No.:	85	86	87	88	89	90	91	92	93	94
Microns:	79	78½	78	77	76	75	74	73	72½	72
No.:	95	96	97	98	99	100				
Microns:	71	70½	70	69	68½	68				

By choosing, in this table, the "No." in proportion to the number of dents, the reeds might be so constructed that they all have the same clear space, which ought to be in the neighborhood of 0.7 Ctm.

But it is not quite easy to have such reeds made by the reed-makers who are accustomed to employ certain wires for certain "No." thus producing reeds whose clear space varies from 4 to 6.3 millimeters to the centimeter. They are led by no distinct principle in this regard, but simply by the fact that it is more convenient for them to fix thicker dents with thinner wires than vice versa.

If a reed with forty dents No. 90 to the centimeter is ordered of them, they generally will deliver dents No. 70, trusting that they cannot be controlled. This, however, is quite easy with the aid of our table, and of a slide-gauge indicating 0.1 millimeter, if one orders a little bit more of length than wanted and takes off 5-10 dents, measuring their total size; or with the help of a microscope, without taking off any dents.

If one protests against the "No." not in accordance with that ordered they will reply that dents No. 90 are not durable enough, and on the question why dents 90 should be less durable in a reed with 90 dents than in one with 50 to the centimeter, they will offer other excuses, declaring finally: "It is impossible."

It is a fact that fine reeds are less lasting, but this will keep no manufacturer from employing them, seeing that they furnish a finer tissue. It appears, more-

TABLE OF CLEAR SPACES

Dents per ctm.:	18	19	20	21	22	23	24	25
Clear space, $\frac{m/m}{100}$:	703	700	697	694	691	688	685	682
No. of dents:	41	43	45	47	48	50	51	53
Dents per ctm.:	26	27	28	29	30	31	32	33
Clear space $\frac{m/m}{100}$:	679	676	673	670	667	664	661	658
No. of dents:	55	57	58	60	61	62	63	65
Dents per ctm.:	34	35	36	37	38	39	40	41
Clear space $\frac{m/m}{100}$:	655	652	649	646	643	640	637	634
No. of dents:	66	68	69	71	72	73	74	76
Dents per ctm.:	42	43	44	45	46	47	48	49
Clear space $\frac{m/m}{100}$:	631	628	625	622	619	616	613	610
No. of dents:	77	78	79	80	81	82	83	84
Dents per ctm.:	50	51	52	53	54	55	56	57
Clear space $\frac{m/m}{100}$:	607	604	601	598	595	592	589	586
No. of dents:	85	86	88	89	90	91	92	93
Dents per ctm.:	58	59	60	61	62	63	64	
Clear space $\frac{m/m}{100}$:	583	580	577	574	571	568	565	
No. of dents:	94	95	96	97	98	99	100	

over, that the making of fine reeds with a clear space of 0.7 Ctm. offers some difficulties. Until these are overcome we must confine ourselves to calculating a series whose clear spaces diminish, with the fineness, from 0.7 to 0.565 Ctm.

Comparing the space demanded by the respective warp (see Chapter XX) to that left to it, we find out the limit where the warp-threads begin to be pressed together and therefore are exposed to increased friction. From this moment the difficulties increase rapidly and the quality of silk must get proportionally better, if weaving is to be at all possible.

On those circumstances is based the table of Chapter VIII (cohesion), the result of three years' studies and work.

Chapter XXIII

SOME HINTS ABOUT POWER WEAVING

No silk is good enough if the loom is not in perfect order.

While I was still at the inception of my studies the results obtained were sometimes apparently contradicted by experience. A bale of silk which had been classified as good under test, presented in the weaving unexpected difficulties which were sometimes hard to overcome.

But in ninety-nine out of one hundred cases they lay in the loom itself, and by finding their origin and trying to avoid them thereafter, I was led to establish the following rules which are not to be found in weaving manuals :—

(1) Silk on the way from the winding to the weaving should become more humid rather than dry, to prevent its shortening. (See Chapter XXIV.)

(2) The warper should thoroughly clean the warp. (See Chapter VI.)

(3) In tying the silk the knots should be drawn sharp.

(4) The warper should work with the same number of reed that the warp will have on the loom.

(5) Care must be taken to keep every thread in its proper crossing.

(6) The warp should not be beamed under too heavy tension.

(7) The warp beam should revolve easily.

(8) The tension rope should glide easily and without jerks.

(9) The whip-roll should keep the warp in horizontal position.

(10) The harness should be clean, free from roughness, and not matted.

(11) The harness should consist of fine threads.

(12) Harness knots must not be too thick.

(13) Too much tightening of the harness should be avoided.

(14) The distance between the harness and slay should not be more than two centimeters.

(15) The first and last shafts should form an equally high shed, the whole harness forming as low a shed as possible.

(16) The shafts should change: With light count, when the slay is at one centimeter's distance from the tissue; with heavy counts, when the slay touches the tissue.

(17) The reeds should move freely in the frame.

(18) Reed dents should be made of soft blue steel.

(19) Dents should be free from rust and too much wear.

(20) They should not be made too tight.

(21) Nor should they be too voluminous. (See Chapter XXII.)

(22) There should not be the slightest scar on the shuttle.

(23) Shuttle points should neither be too sharp nor too blunt.

(24) The bore-hole of the pickers should not be too deep.

(25) The picking should be as soft as possible.

(26) The picking should be effective the moment when the crank is at its lowest point.

(27) The slay's course should not be too hard.

(28) The slay should touch the warp only at its hindermost position.

(29) The slay should form the same angle with the reed as the shuttle shows.

(30) The oscillation of the slay should not exceed eleven centimeters.

(31) The loom should be so firmly fixed that it is not visibly shaken by its operation.

Chapter XXIV

SOAKING

THROWSTERS have a waste in the course of their work which, as we saw in Chapter XIX, varies between 1-3 per cent. and 3 per cent., and it ought to be allowed to them to put it into account. This natural claim was, however, always refused by their customers, and the throwsters found an expedient in weighting the silk.

The following table shows the weighting allowed in Lyons, if the customer has not made the condition "sans charge." The boiling-off percentage of raw silk is established according to the averages of the essays made by the Stagionature Anonima, Milan, from 1901 to 1910.

	Boiling-off per cent. Raw silk.	Allowed for thrown silk.
Japan		
Kakedah	17.95	20
Filature	18.44	
China		
Filature	18.30	22
Tsatlee	19.85	
Central Asia	21.75	22
Levante	22.26	24
Canton	22.70	25
Italy	23.18	26
Syrie	24.77	27
Average	21.00	23

We see that the charge is meant merely as a compensation for the loss of raw material. Compare this with the request made by a throwster of the Laboratory Serivalor: "I know how to weight up to 10 per cent., but how must I manage to arrive at a greater weighting?" So far the soaking, or rather the applying, of a mixture of soap and greasy material would be justified to a certain degree, but it is hardly comprehensible how it could have become an axiom that this procedure is useful or even necessary for good winding!

On the contrary, it is more or less detrimental to the quality of silk and not even useful for the winding. During the first hour, as long as the skeins are thoroughly wet, the winding, in fact, goes on much easier; after this time, however, the skeins, making about 100

revolutions a minute, are completely dry again and the threads are sure to stick together much more than if they had been "rubbed out" and not soaked; the consequences are numerous breaks. If somebody should believe that the drying of the skeins can be avoided by humid air in the workrooms, I can tell him that this is not the case even with 90 per cent. of humidity, while already with 75 per cent. all the metallic parts of machinery, etc., get rusty. The humidity, in fact, should not exceed 70 per cent., as this is sufficient.

But in which way is the degree of humidity ascertained at all? Generally by means of a hair-hygrometer, which, however, does not remain reliable for a long time, if not very carefully kept in order and moreover controlled by a psychrometer. This latter instrument, however, is hardly to be found anywhere, and when it exists it is out of order as well and not provided with a regulator of the current of air. It is a fact, therefore, that in general the real humidity of the workrooms is not known and that many theories based on this uncertain factor must be wrong.

It is a very instructive experiment to have a wet silk thread wound tightly around a roll of hard paper and to have it quickly dried afterwards. The thread, lengthened by the winding in wet state, tries to contract itself to its former length and being hindered by the paper roll, simply crushes it. But, of course, it cannot crush a wooden bobbin, and the consequence is

that it is the thread which must yield, that is to say, its molecules will glide along each other, and the thread loses much of its strength and has even some of its fibers split. I have seen good Italian silk on such bobbins which looked like the worst Bengal and which in the upper layers (that were stretched more the thicker the bobbin became) had a breaking-length of only fifteen kilometers; gradually the layers improved to twenty, twenty-five, thirty, and the innermost layers had a breaking length of thirty-five kilometers.

It would be possible to solve the hard parts of the skein by wetting them, and to dry them afterwards in a way that they cannot stick together again, but here is not the place to describe this procedure.

I know important throwsters, withal, who keep strictly to the method of dry winding.

Their Japan trams are justly well renowned, and they easily obtain prices which make up for the loss by waste.

My advice is to wind in the dry way after having taught the hands to "rub out" well the hard parts of the skein—and to accept only those thrown silks that are not soaked in the winding. These are easily recognizable, for they show the natural luster, while the weighted silk has a dull, greasy aspect.

Moreover the task of the dyer is easier by receiving pure silk instead of a mixture of silk, soap and grease of unknown chemical composition, which in the boiling-off sometimes undergoes unforeseen changes.

In chapter XVI I said that "lousiness" is the dyer's fault; I must add, however that he can be made responsible for it only if he has received unweighted, dryly wound silk. It is his duty to treat the silk with all the care and skill of his craft, but he cannot be obliged to have it chemically analyzed before and to free it from the alien materials with which it was covered in the throwing.

Chapter XXV

THE CALCULATION OF GENERAL EXPENSES

THE method of calculating the expenses now in general use in almost the whole textile industry is wrong, as can be seen from the following example:

A manufacturer whose sale in the last year was a million dollars and whose general expenses were $100,000, will base his calculation for the following year on 10 per cent. of the general expenses. Suppose his sales in the last year were:

½ million yards of an article at $1.00 per yd. = $500,000, and
2½ million yards of an article at .20 per yd. = $500,000,

in reality his general expenses for the first article were not ten cents per yard, but *less* than that, for the second not two cents, but *more* than that. This will become evident if we suppose that in the following year fashion has changed, so that the manufacturer is compelled to abandon the better article altogether and

to limit his whole production to the cheaper one. In order to gain the general expenses remaining unaltered, he would have to produce 5,000,000 yards of the lower article, and it is evident that the same looms which produced last year 500,000 yards could not produce 2,500,000 this year.

The error can be avoided by calculating the general expenses in proportion to the weaving wages.

Suppose that in our case the weaving wages had been $50,000, the general expenses must be calculated at the rate of 200 per cent. of the former.

It is not sufficient, however, to base this proportion on a summary statistic, but it is necessary to contemplate various details. For also the general expenses must be considered as different for such articles which are produced on one loom, in comparison with those for the manufacturing of which one weaver suffices for two looms. General expenses are, moreover, higher for fancy articles than for plain ones, higher for jacquards than for shed-fancies, finally higher for orders than for stock goods, and the more so the smaller quantity ordered per color and per pattern is.

It is necessary, therefore, to have these statistics made by a good calculator. As an example we give the following table which fifteen years ago served well for a mill of 400 power looms, with low wages for piecework, but also low selling expenses, the production being sold to a few wholesale firms without traveling.

General expenses in per cent. of weaving wages:

	Production on two looms.	Production on one loom.
Plain articles, on stock	160	
Plain articles, on order	170	115
Fancies on order	180	120
Jacquards on order	200	135

Extra: 20 francs = $4 per loom for putting on a new pattern or a new binding. Calculations of the cost price by the method explained here remain unshaken by variations in the grouping of the articles produced during the year, and avoid disagreeable surprises in the year's balance.

Chapter XXVI

CALCULATION OF PARITIES

BETWEEN LIRE PER KILO, IN MILAN, CASH BASIS, AND DOLLARS PER POUND. NEW YORK, SIXTY DAYS.

BASIS.

100 Kilos, in Milan, @ Lire 45	Lire	4,500.00
Conditioning	"	3.00
Packing	"	10.00
Freight to New York	"	22.00
20 days passage, 60 days payment, = 80 days, 5 per cent. p. a	"	50.00
100 Kilos, New York, 60 days	Lire	4,585.00
1 lb. (453.6 gr.), New York, 60 days	"	20.80
Change: 10 Lire = $2, 1 lb., N. Y. 60 days		$4.16

If, then, the change in New York is $2 for ten lire, the parity is to be calculated by multiplying the price in lire with 0.0925. The following table shows the multiplicators (leaving aside the decimal point) for the other changes.

Change 10 Lire=cents				200	199	198	197	196	195	194	193
Multiplicator				925	920	915	910	906	901	897	892
192	191	190	189	188	187	186	185	184	183	182	181
887	883	878	873	869	864	860	855	850	846	841	837
Change 10 Lire=cents				180	179	178	177	176	175	174	173
Multiplicator				832	827	823	828	813	809	804	800
172	171	170	169	168	167	166	165	164	163	162	161
795	790	786	781	776	772	767	763	758	753	749	744

CONCLUSION

THIS book was written during the great European war. After having begun to write it in Milan, the author was compelled to finish it in a Swiss village, without the help of his notes, collected during many years. But for this impediment its statistical material would have been richer. I suppose, however, that the reader will have got enough of figures, as it is, and I hope they will not make him lose his courage. If he cannot master the sometimes difficult subject right away, a second perusal will make him better acquainted with it, and may be he will acknowledge the author's endeavor to speak "truth and nothing but truth."

Perhaps he will also come to the conclusion that more can be learned from books than from teachers and more by self-thinking than from books.

Right classification of silk, of course, can be taught as little as swimming by a book, but if the reader has been freed from many prejudices and errors prevailing in our industry, he will certainly not regret his trouble, as little as has the author himself, who has been studying these matters for more than twenty years.

ADOLF ROSENZWEIG.

LUGANO-SORENGO, 1915.

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